OCT 30 '37
INDEXED

Philosophy of Science

VOLUME 4, NUMBER 4

Published Quarterly by The Williams & Wilkins Company, Baltimore, Maryland

October, 1937

PHILOSOPHY OF SCIENCE

ORGAN OF THE PHILOSOPHY OF SCIENCE ASSOCIATION

The Editorial Board

WILLIAM MARIAS MALISOFF, *Editor*
New York, N. Y.

ERIC TEMPLE BELL
ALBERT E. BLUMBERG
RUDOLF CARNAP
MORRIS RAPHAEL COHEN
W. W. COOK
HERBERT FEIGL
K. S. LASHLEY
HENRY MARGENAU
H. J. MULLER
L. SUSAN STEBBING
DIRK J. STRUIK
ALEXANDER WEINSTEIN

The Advisory Board

EDGAR ALTENBURG
G. D. BIRKHOFF
NIELS BOHR
MARCEL BOLL
P. W. BRIDGMAN
K. T. COMPTON
E. U. CONDON
W. J. CROZIER
JOHN DEWEY
F. G. DONNAN
L. C. DUNN
ALBERT EINSTEIN
R. A. FISHER
J. B. S. HALDANE
R. G. HARRISON
H. HENCKY
H. S. JENNINGS
K. KOFFKA
W. KÖHLER
K. LANDSTEINER
I. LANGMUIR
RALPH LILLIE
A. O. LOVEJOY
W. MAYER
W. P. MONTAGUE
T. H. MORGAN
CHAS. W. MORRIS
JOSEPH NEEDHAM
J. VON NEUMANN
OTTO NEURATH
F. S. C. NORTHROP
H. A. OVERSTREET
M. POLANYI
G. Y. RAINICH
N. RASHEVSKY
H. REICHENBACH
O. L. REISER
O. K. RICE
A. E. RUARK
GEORGE SARTON
C. E. SEASHORE
H. SHAPLEY
E. A. SINGER, JR.
HENRY B. SMITH
W. F. G. SWANN
R. C. TOLMAN
H. UREY
M. S. VALLARTA
PAUL WEISS
GERALD WENDT
HERMANN WEYL
A. N. WHITEHEAD
N. WIENER
SEWALL WRIGHT
F. ZWICKY

Entered as second class matter November 25, 1933, at the Post Office, at Baltimore, Maryland, under the Act of March 3, 1879.

Made in United States of America

Subscription price $5.00 per volume. Single numbers, $1.25

Philosophy of Science

VOL. 4 October, 1937 NO. 4

CONTENTS

DesCartes' Method of Doubt. Virgil C. Aldrich - 395
On the Troublesome "X." R. G. Tugwell - - - 412
Vagueness. Max Black - - - - - - - - - 427
A Critique of Operationalism in Physics. R. B. Lindsay - - - - - - - - - - - - - 456
Symbolic Logic and "Embedding Language." Edward O. Sisson - - - - - - - - - 471
The Method of Postulates. Edward V. Huntington - - - - - - - - - - - - - 482
Discussion (Correspondence) - - - - - - - 496
Reviews and Notes - - - - - - - - - - - 499

Copyright 1937, The Williams & Wilkins Company

An Introduction to the PHILOSOPHY OF SCIENCE

By

A. Cornelius Benjamin

UNIVERSITY OF CHICAGO

This book attempts for the first time to construct the fundamental concepts of a philosophy of science from the great amount of current literature on this subject. It defines the general field of philosophy of science, indicates its main problems, and discusses the main types of solutions. The first part of the book deals with problems in the logic of science, giving an analysis of scientific method, defining the nature of scientific symbols and their use, and classifying the basic scientific points of view. This is followed by an examination of fundamental scientific concepts such as order, number, space, time, motion, etc. from an operational point of view. The final section discusses three outstanding speculative problems of science: classification, human freedom, and the nature of reality.

$3.50 **MACMILLAN, NEW YORK**

Philosophy of Science

VOL. 4 October, 1937 NO. 4

DesCartes' Method of Doubt[1]

BY

VIRGIL C. ALDRICH

LORD ACTON, in his letter to the contributors to the *Cambridge Modern History*, wrote: "By Universal History, I understand that which is distinct from the combined histories of all countries . . . and is not a burden on the memory but an illumination of the soul." If we replace "history" by the more general term "knowledge," we get the statement of an ideal cherished by the great men of every age—those lonely pioneers to whom book-learning is an intellectual gloom more treacherous than the natural gloom of night, since it fools men into thinking that it is a light by which men can see. The pioneers in thought, or indeed in any field, have made a secret treaty with nature. They smile with her at the robed and tassled scholar who, (Goethe's words) "Bewundrung von Kindern und Affen," can (DesCartes speaks) "lecture plausibly about anything and get himself admired by the less learned." Those pioneers, those men who—as someone has said—have thought according to no one and have made the human race think according to them, see by an intuitive insight, an illumination of the soul. Einstein, in his prologue to Planck's book *Where is Science Going?*, writes: "There is only the way of intuition. . . . The state of mind which furnishes the driving power here resembles that of the devotee or the lover. The long-sustained effort is not inspired by any set plan or purpose. It arises from a hunger of the soul." We have not quoted a poet, but the greatest of mathematical physicists.

[1] Public lecture delivered in 1937 at the Rice Institute in honor of the three-hundredth birth-year of the *Discours de la Méthode*.

There are perhaps two sources of the "illumination of a soul," the light of reason and the light of faith. The light of reason is a *lumen naturale*, a light of nature, a secular light. The man who is able to see by this light understands things. He demonstrates them in the context of their *rationale* or intelligible structure. The systematic statement of his findings by the natural light is called science, in the broad sense of the term. Anything lit up by the *lumen naturale* is intellectually comprehensible. Classical Greek theory was formulated in this light of reason. Plato devised a method whereby a man could, after years of self-discipline, learn to see the real nature of things in the bleak rays of the sun of Universal Truth, by the naked eye of reason. He praised the science of number as a means to this end. And his famous pupil was the first to give us a formal logic—the science of what you cannot say without making nonsense.

But, some three hundred years later, there appeared one who taught men to live by another light—the light of Faith. This is a supernatural, heavenly, sacred light, a light of grace (*lumen gratiae*), a ghostly light—in the sense of Holy "Ghost" or "ghostly father." To such a light men pray in Newman's words, "Lead, Kindly Light." Milton calls it a "dim religious light." And so men entered what we call the Dark Ages. But we should in fairness remember that these Ages are dark to one who is accustomed to see by the light of reason. Those who, on the contrary, are accustomed to seeing by the "dim religious light" tell us that our twentieth century, brilliant with science and reason, is the darkest of all ages. Maybe they are right, though I can find no way to settle the issue one way or the other. It seems to make sense, however, to argue that, just as certain stones in ultra-violet light reveal fluorescent colors not visible in the garish light of day, so there may be a "revelation" of features of reality in the twilight of faith which could not possibly occur in the light of reason.

The Dark Ages culminated in an attempt of the Scholastics—Abelard, Albert the Great, Thomas Aquinas—to introduce more natural light into religious doctrine. They led the pagan child Reason into the Temple of Faith, and gave it orders to play there as it pleased—without ever leaving the temple. This was a studied attempt to schematize the Catholic faith by recourse to Aristotelian sources. It produced an intricate and unstable blending of the two lights of heaven and nature. Duns Scotus and William of Ockham logically analyzed the blend, concluding that many of the seemingly rational doctrines of the schoolmen could be known only in the light of faith. But the Thomist world-

view had become the classic and final creed of the Catholic Church, and it was mainly Thomism that was taught at the beginning of the seventeenth century by Jesuits at the college of La Flèche in France.

A very general separation of the two kinds of "illumination of the soul" had been achieved in practice during and since the Renaissance. Men had begun to see nature again by the secular light of nature. This evidenced itself not only in the fine arts, but also in the renewed efforts of earthly states to break away from the control of the heavenly church, in a growing geographical curiosity which resulted in the circumnavigation of the globe, and in the daring research by scientists such as Copernicus and Galileo, who were made to tremble by the still-powerful church for what they saw in the plain radiance of the *lumen naturale*. We might mention the Reformation and Luther's religious individualism also, but not as an example of turning again to the light of Reason. Luther's aim was, rather, simply to purify or clarify the light of Faith, by taking the intellectual schema out of its objects. He did indeed advocate a separation of the two ways of seeing, but because he would have little to do with the purely rational way.

So we observe that, during the four hundred years between the thirteenth century—Roger Bacon—and the third decade of the seventeenth century, there occurred, *in practice*, a very general separation of Faith and Reason, Theology and Science, the sacred and the secular, a practical or working separation which, on the continent at least—this leaves out Francis Bacon[2]—had received no wholesale or systematic formulation in theory, with an eye to the new back-to-nature movement. A growing need was felt for someone who could give, theoretically, the light of nature and what it revealed a firm, articulated place in the new scheme of things, without doing too much violence to the old. The task fell to the Frenchman René DesCartes.

It seemed to DesCartes that the incoherence of the philosophy and even the special sciences in his day was due to the lack of a method for the wise regulation of reason in the search for truth. There was plenty of genius, but unregulated genius is just as apt to go hugely wrong as right. So he wrote a *Discourse on Method* which first appeared off a Dutch printing-press three hundred years ago June the eighth (about a year after it was written) when the author was forty-one years

[2] Though Bacon formulates an inductive method of the sciences and DesCartes, later, a deductive one, DesCartes' formulation savors more of actual observational science, because (1) he was a more alert observer of natural phenomena ("meteors") than Bacon, and (2) unlike Bacon, he gave mathematics its proper prominent place in scientific procedure.

old. This brief text, accompanied by three essays on Dioptrics, Meteors and Geometry, is a sort of intellectual log-book, a brief sketch not only of the Cartesian method and the world-view consequent upon it, but also of René the man. One of his friends, Mersenne, advised him to give it the more dignified name of "Treatise on Method," to which DesCartes replied that in it he was simply chatting with his reader, not writing a formal treatise. And to his friend Balzac he wrote that he once thought of calling it "The Story of My Mind." By far the kinder thing for me to do here would be to read you that whole log-book of intellectual travel—kinder to you and to DesCartes. That would take about three hours, a three hours which would seem shorter than this one long hour of Sunday afternoon lecturing and would draw aside the veil of three hundred years to give us an ample view of DesCartes. But the proper—if not the kind—thing for me to do is to give you a very unfair glimpse of him by reading to you my own version of him and of his method of doubt. . . .

About fifty years after the death of those two great rebels Copernicus and Luther and four years before Bruno was burned in 1600 for having denied that our earth is the hub of the wheel of things, and while Brahe, Kepler and Galileo were in their prime, DesCartes was born in Touraine, France, of high middle-class propertied parents. From his mother, who died shortly after his birth, he inherited, as he said, a dry cough and a pale skin. Doctors said he would die young, but a faithful old nurse and an incredibly prudent treatment by his Jesuit tutors in the college of La Flèche saw him across the danger zone to the age of twenty, after which he enjoyed for thirty-four years the hardihood of a sound body. In college, he was allowed to stay abed late into the day—not sleeping, but thinking. The results of these bed-incubated meditations on physics, arithmetic, algebra and geometry surprised and pleased his tutors, though they were all good Thomists, as we have seen. In 1610, Galileo observed (through his newly-made telescope) that Jupiter had moons; and when news of the event reached the inmates of La Flèche a year later, towards the end of DesCartes' stay there, they took time off to celebrate it.

But DesCartes' early ill-health bred in him a life-long preoccupation with the science of medicine and health, and with the possible uses to which all the sciences could be put.[3] He says, in a dozen different ways

[3] Towards the end of his life (1648), he thought of organizing "une sorte d'École des Arts et Métiers, fondée sur le principe d'une étroite collaboration entre les savants et les artisans." See Gilson: *René DesCartes, Discours de la Méthode, Texte et Commentaire;* p. 446.

throughout his life, that the aim of *all* his studies and reflections is a knowledge of how to procure and secure good health for mankind, which he calls the *sine qua non* of all other human goods. Even in connection with an *ethical* treatise on the passions, addressed to Princess Elizabeth, daughter of the king of Bohemia, he writes her in a letter that health and the moral conduct consequent upon it are largely engendered by a simple diet of fresh, purging food.[4] He was as touchy about his own physical well-being as a "rich man with the gout." And when an acquaintance came to see him and wondered what he read, DesCartes pointed with his knife at sundry parts of animal bodies he had brought home from the butcher's, saying, "These are my books."

But this was only one page of "the great book of the world" to which DesCartes turned after leaving La Flèche at the age of sixteen. There, what had impressed him most was arithmetic and geometry, for their rigor and clearness. But the Thomistic philosophy of nature, also dished out there, was to this lover of clear and distinct ideas, a stew of vague and colorful imagery—a child-psychology dignified by mere words into the appearance of a science of nature. It was a systematic defense of the world as it appears to the child. And since nature, from this thoughtless point of view, is animated with the sensory qualities of color, sound, heat, etc., so, to the orthodox Thomists of DesCartes' era, the event of, say, a piece of wood burning was to be *physically* and technically explained by saying that there are present in the wood the Form of fire, the Real Quality of heat, and the Action of burning. This, for DesCartes with his bent for mathematical analysis, was nothing short of animism and verbal camouflage, which joined sensory and occult qualities to material substances like so many little souls to bodies, in a wholly incomprehensible manner.[5] He thought that a man brought up on such intellectual fare is less likely to find out the truth about himself or the world than an uneducated man with a clear brain who knows only the dialect of Brittany. Furthermore, natural scientists had begun to use mathematics in the analysis and statement of laws of physical processes, without a metaphysical justification of their procedure. Living in and observing this state of affairs, together with certain dreams he had at the age of nineteen, early filled DesCartes with the sense of a mission. He believed himself chosen by Providence singly to show how much more can be known, and *certainly* known, by the *lumière naturelle*, the naked light of nature, than was commonly thought

[4] *Oeuvres de DesCartes* (Adam and Tannery edition), Vol. IV, p. 625.

[5] See the significant excerpt in a letter to Mersenne, op cit., Vol. III, p. 648.

—just the opposite of what Scotus and Ockham tried to show. Even first principles, indeed *chiefly* first principles, concerning the nature of the soul of man, the physical world, and the existence of God, were to be known in the secular light of reason, and ramified by strict deduction into a world-view. His originality consists partly in this. The custom had been to accept first principles on faith, and then to run them through the mill of syllogistic logic to see what would come out at the other end. Curious things came out, results which DesCartes believed were the freakish fruit of trying to see by the ghostly light of faith that which falls outside its supernatural sphere of illumination. Not that he thought he could get along without the Kindly Light, which he learned to love as a youth, and indeed to live by. He was a devout Catholic unto death. He even said that the objects of faith appear more clearly in their own way than anything in the natural light. But the genuine DesCartes revealed himself in a statement to his friend Huygens, to the effect that what he saw by the light of nature moved him much more than what mere faith taught him.[6]

So DesCartes turned from the books and tutors of standardized education to the great book of the world, and after the age of twenty did little reading except in the latter. As a man of leisure, a soldier and an observant and pensive traveller (in Germany, Italy, Holland and his own France) he browsed through its pages, in an effort to erase from his mind what was mere pedantry, prejudice or verbiage—mere book-learning. Fortunately, he had the private means to satisfy this restless craving of genius to spend long periods looking directly into the face of nature and human nature, instead of seeing it, or himself having to represent it as a teacher, in the dusty warped mirrors of books. (The kindest and wisest thing the public can do for a man of genius, says DesCartes, is to pay all his expenses and "du reste, empêcher que son loisir ne lui fût ôté par l'importunité de personne." Op. cit., Vol. VI, p. 73.) As a result, DesCartes never became learned. He was ignorant of, and content to ignore, much in the history of thought which it would have profited him to know. This naïve contempt for book-recorded knowledge of the past was reflected in his disciple Malebranche, who said simply: our reverend parents Adam and Eve knew no history; why should we know any? And when his friends generously pointed out to him that in certain of his theories he had behind him the authority of St. Augustine or some other dead and buried big-wig, he would petulantly say that his arguments were good enough to stand on their

[6] Op. cit., Vol. XII, p. 553.

own legs. This attitude was partly conceit, but it was mostly a sign of the pioneer in him, living according to Kipling's principle that he who travels fastest travels alone.

So, having first learned what he could from tutors and books, then from life around him, he turned finally to see what he could find in himself—usually the most difficult and yet, where there is something in the self to engage one, the most profitable operation. He was then a soldier, returning alone towards the army in the winter of 1619 from the coronation of the Emperor Ferdinand of Bohemia. He stopped, probably at the village of Ulm, and shut himself up for a day beside a stove, and there, free from physical needs, passions and talkative friends (and radios!), he fell into his famous meditation, during which he outlined in his mind the method of doubt, together with the theory of reality disclosed by its use. About seventeen years later, when he had learned to live and think by this method, he wrote it for publication in his native French, after he had settled in Holland. Having observed the reaction to this "story" and profited by some criticism, he elaborated his method in a much more formal Latin treatise called the *Meditations*, which appeared four years after the *Discourse*, and was addressed mainly to the "deans and doctors of the sacred Faculty of Theology of Paris." His publishers complained about the small sale of these books, and DesCartes himself, although he made many converts, was dampened by a general blindness on the part of his readers to the absolute *certainty* of the truths he had recorded in them. But let us turn now to the analysis of the Cartesian method of *doubt*.

We have just mentioned the words "certainty" and "doubt" in the same breath. The reason for this is that DesCartes' method is perhaps better named the "method of certainty" than the "method of doubt." Practically all the rules he formulates for "the quest for truth by the light of nature"—which is the title of one of his works—have to do with certainties, or describe the ear-marks by which an absolutely certain bit of knowledge is to be recognized. And DesCartes frequently asserts that doubting is less perfect than knowing, or is the sign of a defect in the doubter. So he is not, sophist-like, going to doubt for its own sake, or to show off an agile but half-baked intellect, or simply to get the pleasant feeling of superiority that comes from being unorthodox. I think we may fairly compare Cartesian doubt to unrequited love. It is aware of something wanting, something to consummate it—knowledge in the fullest sense of the term. Or, to put it in another way, just as Pilgrim had to pass through the Valley of the Shadow of Death to enter

Life with a capital "L," so perhaps one has to pass through the shadows of the valley of doubt to arrive at Knowledge with a capital "K." Since one may get lost during this journey, DesCartes asserts that some persons should not attempt it—such as those who can make snap-judgments only and have not the discipline or patience to regulate their lives rationally after leaving the common road of custom, and, on the other hand, those modest, simple souls, who know that the best they can do is to live by social habit or by the opinions of persons more fit for pioneering than they. We might have mentioned a third kind of light, namely, the light of custom or general practice, an "ethical" light in the sense of the *ethos* of a certain people. Some people, perhaps most people, can see only by this light, and DesCartes refers here to such. Never does he scorn this light, nor the living by it. Indeed, his method of doubt prescribes it, as a practical foothold for the doubter during the period of inner, intellectual demolition of prejudice or uncriticized belief. (The reformation DesCartes proposed himself had nothing to do with the overthrow of social orders. Therefore, he could live in and according to them while he was undoing and redoing his inner cognitive experience. He went so far as to say that social reform movements usually entail greater evils than the evils which have developed naturally *pari passu* with the growth of the group.) He hated any public row, so much so that he delayed till after his death the publication of his early physical treatise *Le Monde* when he heard in 1633 that the seventy-year-old Galileo had been forced publicly to get down on his knees and disavow the Copernican thesis that the earth moves. (The Church had yet to learn that if you want the earth stationary and flat, the scientist who knows it moves and is round, and why, is certainly not the man you want to hound to death, since he, in the end, may make the earth stationary and flat for you, if you ask him nicely.) DesCartes was not inclined to make a public figure or martyr of himself because he had something more important to do—the improvement of his private intellectual life, and neither fame nor prison nor the stake could be of very great help in the furthering of this end.

(But to get back to doubting. DesCartes had observed, on the one hand, the chaos of customs and theories founded on prejudice, and also, on the other, the coercive orderliness and clarity of mathematics. Moreover, he had previously generalized the piece-meal geometry of his day into a systematic whole. This led him to believe that he could similarly generalize still further, until the whole of human knowledge had been systematized, *in ordine geometrico*, into an indubitable unit, a

sort of Universal Science deductively generated from a few perfectly clear and distinct primitive (undefined) ideas. Just as Russell and Whitehead have in our own day deduced the theory of number from logic, and geometry from the theory of number, thus showing, in their *Principia Mathematica*, that all three comprise one deductive system, so DesCartes dreamt of linking up *all* the sciences into a logically ordered whole, so clearly illuminated by the light of nature or reason, that no rational creature could possibly doubt in its presence. One could not see such an object in such a light without believing it.

Now, the best way to clear the air of doubtful entities is to doubt everything which is dubitable. This process relegates those doubtful entities to the sphere of outer darkness where they belong, and leaves the *lumière naturelle* free to shine clearly on the remaining indubitables. The result is what we have called an "illumination of the soul," by the light of nature. DesCartes, like Socrates, believed that this natural light shines evenly in all human beings, such that the only thing required to make them all agree about truth and reality is—if I may put it thus—to remove from their mind's eye the cataract of prejudice or the lethargy of habit. Then, where the eye is single, there is a single vision of the truth, coercive upon all.

So we arrange all the objects of experience before us, in order to subject them to systematic doubt. Just as we remove all the apples from a basket to separate the rotten from the good and put back only the good, so are we going, by doubting, to empty our minds of all beliefs, in order to retain only such as are theoretically sound—remembering meanwhile that this rehearsal is to be an inner intellectual one only, and not to register itself in overt conduct. First, the whole set of habitual beliefs, based mainly on the testimony of the senses, goes by the board. The senses are known to deceive us at times, and we are never *quite* sure whether we are awake and engaged in real bodily motion, or simply dreaming, such that everything which seems to be the case in ordinary work-a-day moments may not really be the case. I am not even certain that I am really lecturing to you now, since I have had a dream—or, rather, a nightmare—to the same effect, during which I was just as unhappy as I am now.

As for the scattered results of the applied or inductive sciences, they are at best only probable. And DesCartes' whole aim is to employ a method of certainty through doubt. He had little patience with probable reasoning. Once in 1628, when Chandoux (a gentleman who, like DesCartes found the scholastic philosophy wanting) attempted a criti-

cism and revision of it in public by uncertain arguments but with a very fine show of wit, DesCartes did not join in the general applause. The Cardinal Bérulle observed his silence, and asked him for the reason. DesCartes got up and, in his usual feeble voice and quiet manner, proved that, by merely probable reasoning, a belief taken as true can be shown false and taken as false can be shown true. The Cardinal, indeed that whole audience in the home of the papal nuncio, was so moved by the demonstration that he, Bérulle, took DesCartes aside after the meeting and made him feel that he had a serious duty to benefit mankind by writing for publication—just the friendly nod of encouragement the backward and indolent DesCartes needed, who, according to his own statement, was no maker of books.

But, now, what of the abstract deductive sciences of arithmetic or geometry? Are they indubitable? Can one doubt that two and two make four, or that the sum of the interior angles of a triangle is two right angles? DesCartes was at first inclined to answer "Yes, it is dubitable," arguing that there may be no real couples or triangles in nature. But, in the face of various objections,[7] he finally argued the absolute certainty of such propositions, which were so luminous in the natural light of reason that one could not possibly doubt *during a direct intuition of them*. They then evidence themselves or are self-evident. But the equation 2 plus 2 equals 4 is by no means the science of arithmetic. Arithmetic or geometry as sciences are elaborate systems. And since we cannot get an immediate intuition of the whole system of arithmetic or geometry at once, but only *seriatim*, *we have to trust our memories* as to what went before in the deduction of some unobvious principle. And there's the rub. Memory is treacherous. What we remember is dubitable. So we doubt it, and that relegates all the deductive sciences as *systems* of knowledge into the limbo of doubtful entities. . . .

A result of this wholesale application of the method of doubt is, so far, that no bodies exist, for all we *know*, since such things as chairs, tables, mountains, even parts of our own bodies, often seem to exist and have certain characteristics when they really do not. (When a man loses a leg, it often feels as if he still had it.) It is just *possible*, therefore, that there are no bodies, not even our own. This conclusion,

[7] DesCartes, in his earlier period, inferred the existence of God from clear and distinct ideas, then inferred the veracity of clear and distinct ideas from the existence of God. This argument became known as the "Cartesian circle." That DesCartes profited by the objections brought against it is evident from an inspection of the thirteenth principle, Part One, of his *Principes de la Philosophie* (1647); op. cit., Vol. IX.

coupled with another proposition we are about to arrive at, will have a startling result. Let us see what this other proposition is.

Granting that I am thoroughly deceived in all that seems to me to be the case, what then? It follows that I, in so far as I am deceived, am something. I exist. This is St. Augustine's *fallor, ergo sum.*[8] DesCartes infers his existence from the doubting or thinking process. One cannot doubt that one exists as a doubter. One cannot think that one does not exist as a thinker. Therefore, quite certainly, *Je pense, donc je suis; cogito, ergo sum.* So I am certain of my existence as a thinking somewhat.

Now couple this statement with the one about the doubtful existence of bodies, and we get the promised startling argument as to the nature of the "I" which thinks. Here is the argument: I am certain that I exist, but I am not certain that any *body* exists; therefore I am not a body. DesCartes did not cast the argument in this form, but I believe it shows the nerve of the argument even better than the meandering way in which he states it. He takes this, supplemented with other evidence, as certain proof of the real distinction between his mind and his body and as an absolute guarantee of the existence of his mind as a thinking substance. The *lumière naturelle* shows that the thinking-process cannot occur by itself or in *vacuo*, but is characteristic of a thinking thing, which latter he calls the mind, soul, or spiritual substance. So DesCartes has found what he was looking for—one fixed point of certainty in the universe, and now, by the lever of strict deduction, he is going to move the whole realm of theory.

Having arrived at an indubitable first principle, namely, that he exists as a mind whose necessary essence or stuff is thinking, DesCartes wonders if such an existent implies the existence of anything else. This question is to be answered by an analysis of the mind or self. Is it the sort of thing that could exist by itself? Certainly not. In the first place, if it had had the power to create itself—which only an all-powerful being can do—it would all the more have been able to make itself without its present limitations. Therefore, it must have been created or caused by something; and, in the end, you get to something which must have been able to create itself. Such an all-powerful being is, by definition, God. Therefore, God exists. This argument is simply a modified form of the orthodox Thomistic argument from a First Cause, and DesCartes subscribes to it partly to mollify and hold his reader. He has more subtle arguments in store. The second proof of the existence of

[8] *De Civitate*, XI, 26.

something besides himself is the "ontological" argument, devised by St. Anselme. DesCartes finds in his mind the concept of an absolutely perfect thing, the *ne plus ultra* of reality. And a part of the very meaning of the concept—as even an atheist would allow—is existence. A non-existent God would not be perfect or real, by lacking the quality of existence. The idea or definition of a *triangle*, though perfectly clear, does not include the existence of the triangle, but you cannot even think the concept of God without thinking that He exists. Therefore, by definition, God exists. Only on pain of making nonsense can you be an avowed atheist. You might as well say that you cannot see the connection between five added to three and the number eight. In the third place, and lastly, we come to the argument originated by DesCartes himself, to the effect that his mind cannot exist alone in the world. Here, he infers the existence of God as the necessary *cause* of his *idea* of God. This involves partly the meaning of the concept, partly the wonderful fact that it exists in his mind. We shall not here follow the subtleties of this argument. In substance, it is simply this: the concept of God is such that nothing inside the finite human mind could be its cause, nor anything outside that mind, except God Himself. And, since the idea of God certainly exists, God, as its real source, must also exist. Furthermore, since nothing I have ever become aware of in sense-perception could possibly generate in me the idea of God, and since I found it ready-made in the "treasure-chest of my mind" (*trésor de mon esprit*), I am forced to conclude that this concept was innate in me, put in me at birth as the stamp or signature of the divine Master Workman whose handiwork I am.

I hope neither DesCartes nor I have wearied you, because we come now to the question as to what, if anything, can be *known* to exist in the physical world of nature, and that is the really interesting issue to DesCartes. Though he makes much initial ado about routing the atheists, and proving that a man's soul is something substantially different from his body, he is a physicist and mechanist at heart, and all these prior metaphysical proofs serve mainly to buttress natural science or give it an assured arena to develop itself in.[9] There is a specious clarity or certainty about DesCartes' arguments regarding the soul and God which savors of the medieval reasoning that he is trying so hard to get away from. But let us see to what use he puts their results.

[9] Though students of DesCartes are more or less familiar with this Gilsonian interpretation of DesCartes, it is still an uncommon one for the layman. See *The Discourse on Method (1637–1937)* by Leon Roth: *Mind*, Jan., 1937.

We had to reject the sciences of arithmetic and geometry—generally, of pure quantity—luminous though they were in the clear light of nature, because we could not trust our memories and because, for all we then knew, there may have been nothing outside our minds that they described. But now we have a guarantee that the *system* of such clear ideas, even though our knowledge of it may lean partly on memory, is true of a physical world outside us. That guarantee is a perfect God, whose existence we have proved. The most careful and honest thinker would be immensely deceived by his Creator if there were no such world of matter and space. But a perfect God, by definition, would not so thoroughly deceive his creatures. Therefore, he who clearly and distinctly conceives nature, in the plain *lumière naturelle*, cannot go wrong. And what is such a painstaking analyst forced to think about nature?

DesCartes' answer shows, finally, that he is not a medievalist. Matter is clearly conceived to the degree that it is exactly measured or quantified. No non-measurable occult qualities inhere in matter, since they cannot be clearly thought. Hence the science of pure quantity or number is the true science. The properties or laws of matter are metrical properties or laws, and to be stated as equations. This means that matter, in essence, is nothing but space with its metrical structure. Quantity and number differ only in thought from that which has quantity and is numbered. And since sensory qualities such as colors or sounds cannot be thus quantified or measured, they must be regarded not as in nature but as occurrences in the mind or as mental in essence. The mind itself is not in space, since it cannot be halved or quartered, i.e., quantified. So you are left with a mathematized physical world, a realm of matter—what DesCartes calls material substance—from which all mentality has been abstracted. Physics is no longer garbled by occult qualities moving matter by secret principles of spook-conduct. Nor is there any purpose *in nature*, only calculable mechanical causation. (Rousseau said that DesCartes had cut the throat of poetry.) Even the bodies of animals and men are thorough-going mechanisms. A man's bosom does not heave with mirth, since laughter is an affair of pulmonary compressions and releases.[10] And a man's heart does not beat out of pity or love or because it has a ghostly Power of Beating (*vis pulsifica*), but because it is a mechanical pump. DesCartes had read Harvey's treatise *De Motu Cordis* four years before the writing of the *Discourse* and, in the main, had approved of it. His chief objection to it was based on a misinterpretation of Harvey's use of the old phrase

[10] See Article CXXIV of *Les Passions de l'Ame*; op. cit., Vol. XI, p. 419.

"vis pulsifica." DesCartes thought Harvey had at this point succumbed to the traditional obscurantism of the sort propounded a century earlier by the medico-philosophical authority Jean Fernel, the "modern Galen," of the College de Coïmbre.[11]

So there is a realm of matter, and there is a realm of mind. None of the properties of the one is a property of the other—though, according to DesCartes, they act upon each other when united in a human organism. *This bifurcation or psycho-physical dualism is what the Cartesian proofs of the absolute distinction between body and mind really aim at, a clearing of the field for physical investigation*—and is what lifts DesCartes out of medievalism and makes him clubable with modern scientists. . . . (Objections have doubtless arisen in your minds to certain points in Des Cartes' general argument.) During his own life they sprang up like weeds about him to mar for him the perfect symmetry of his patiently cultivated garden of thoughts. He did not have much to the point to say about some of them, but, since his active friend Mersenne had circulated his *Meditations* in manuscript among some of the famous men of his time to get their criticisms, DesCartes was obliged to publish the treatise together with the objections and his replies to them. He was sensitive to criticism. His reaction to the most pointed objections was neither genteel nor logical. It was either vitriolic or petulant. Gassendi, for example, the French materialist and hedonist, argued brilliantly that, just as one would have no idea of his own eyes if he had never seen other peoples' eyes or his own in a mirror, so DesCartes, far from having any clear notion of the inner eye of his mind or of his bodiless self, had no idea of it at all, since he had nowise observed any such thing. Hence any argument based on the concept of an immaterial self is a mere intellectual vagary, coercive on no one. And if the mind is not in space and has none of the properties of matter, how does it move the body? DesCartes pouted back at all this saying that the critic was thinking too much in terms of sensory images, and, by the very nature of the case, no visible model of what DesCartes meant could be made. If only Gassendi could get an imageless intellectual intuition of the mind, that is, if the light of nature were not so feeble in him, he would see the case as DesCartes did. Similarly, the famous English materialist Thomas Hobbes objected that man has no idea at all of God, such that if God is nevertheless in some manner an object of knowledge, He is seen not by the dry light of reason or nature, but only in the dim

[11] See Gilson: *Études sur le Rôle de la Pensée Médiévale dans la Formation du Système Cartésien*, p. 52 ff.

religious light of faith. Any reasoning about God is therefore specious —a conclusion similar to the one established by Scotus and Ockham. Furthermore, says Hobbes, DesCartes had reified a process. From the fact of his thinking he had inferred that he had a mind made of the *stuff* of thought. One might as well argue that since he walks, he is made of the stuff of walking. Neither walking or thinking are substances, but mere activities of the human body. And activities are not things. DesCartes replies that thinking is the sort of process which could not characterize matter, and therefore inheres in a different sort of substance, namely, mind. And he believes that Hobbes also suffers from a sensory mode of thinking about God and the human soul.

A group of theologians and philosophers criticized his proof of the absolute difference between the thinking self and the body. As we have seen, DesCartes' argument was: I am certain I exist, but I am not certain that any body exists; therefore, I am not a body. The substance of the criticism was that the premises are misleading in this abridged form. They are true only if revised to read: I am certain that I exist, regardless of what I am; and I am not certain that any body exists, *if I am not a body*. Now, from this it does not follow that I am not a body.

All of which shows that DesCartes' critics had some reason to believe that he had allowed himself to accept and argue from ideas which really were *not* clear and distinct, though his whole predilection was to think clearly. The concepts of existence,[12] reality,[13] truth, thinking, are not as simple or self-evident to intellectual intuition as he maintained—or as, say, the properties of the number "3," which are exhaustively fixed by formal definition. These concepts call urgently for analysis and definition, since their meanings vary according to context. Consider, for example, the meaning of "existence" in the phrase "a mathematical existence-proof" and what it means in the applied sciences. Thus DesCartes' method of intellectual intuition seems to have betrayed him. But we can see why. In dealing with purely formal or abstract entities such as numbers, he had found that he could achieve great rigor and clarity without sensory images. One may have a very definite, universal notion of $\sqrt{2}$ without any mental "picture" of it. This misled

[12] In *The Search After Truth by the Light of Nature*, DesCartes makes Eudoxus say that the idea of existence is only confused by analysis and definition, since it is by nature an immediate datum of intuition. Cambridge Edition of DesCartes' *Works*, Vol. 1, p. 324.

[13] Cf. A. Boyce Gibson: *DesCartes*, pp. 303–304.

him into thinking that he was proceeding just as rigorously in dealing with *concrete* entities such as minds and bodies, even though he could not *imagine* what he was talking about. Thus it could seem to him that he was having a clear intuition when there was really no object of thought at all, or only a vague one.

But, after all, DesCartes said that his method of doubt was devised mainly to train the mind to do its thinking without too much imagery and thereby to attain to moments of imageless, model-less intuition[14] of systematized quantities. Since modern physics, and its mentor mathematics, are largely the fruit of such an exercise and could not progress without it, we should bring to DesCartes not only criticism, but also gratitude.

In the last year of his life, Queen Christina of Sweden asked DesCartes to leave Holland and become her tutor. He went, in the face of a premonition of mishap. The unwise twenty-year-old queen had the fifty-four-year-old philosopher give her lessons in wisdom every morning at five o'clock in the dead black of that northern winter—him who all his life had been used to staying in bed till late into the day. And so they buried him—after quarrelling about the sort of burial he should get because he had dared to turn the search-light of his beloved *lumière naturelle* on God and the immortal soul of man, trying to get clearer and more distinct ideas of them. He whose head had, all his life, been bared to the chastizing baptism of the Light of Nature was deposited in a cemetery for unbaptized infants. Sixteen years later, his remains were removed from the Protestant north to his native Catholic France, and they now lie in Paris in the church of Saint-Germain des Prés.

The Cartesian physical theory of vortices, accounting for motion without using the concept of an occult attractive force between bodies, his simplified mathematical notation and his analytical geometry were the main remnants of DesCartes' scientific work. They early replaced the Thomistic science in the universities of Holland and England, and later in France. But about fifty years after DesCartes' death, Newton's law of gravitation was rapidly making obsolete the theory of vortices, in spite of the protest by Cartesians that it involved the concept of an occult force acting at a distance. As for his psycho-physical dualism,

[14] This is what affiliates him with the Platonists of the "Oratory"—Bérulle, Gibieuf, etc.—and generally with the Pythagorean-Platonic tradition rather than with the Aristotelian.

its legacy was the problem of the relation between body and mind. Though the theory in its original dress is not now widely sanctioned, the problem it raised still baffles the wittiest thinker. As for the *Discourse on Method*, it was not and never will be out-moded, since it is a flawless word-portrait of a great mind needing more than anything else to know the truth—and to know it clearly in the Light of Nature.

Rice Institute, Houston, Texas.

On the Troublesome "X"[1]

BY

R. G. TUGWELL

I

AN UNSEEN and unacknowledged specter, an "X," attends at every economic feast, and stands at the elbow of every modern wiseacre. It confounds the forecaster with seeming malice showing itself only in the event and even then with impish and uncertain effect. It laughs at pretensions to knowledge; it tantalizes all figurers, haunts all generalizers and keeps itself to itself, a shadow, an unknown. But phantom as it is, it is still a force, a potent, active, generative substance, mysterious in genesis, related to a dream, but able to accomplish miracles of reality. For the purpose here it will be called the creative experience.

This activity resides somewhere in what we loosely call the mind. It is and it is not something for which the social psychologist ought to acknowledge analytical responsibility. It operates in the lonely hours of night, on the street or in a railway car as well as in the formal place of study or of experiment. It happens not in many minds but in one mind; yet many minds may have wished it into being, may have set up the terms of its relevance. It is therefore both individual and social.

There seems to have been established an embarrassed conspiracy of silence regarding it, a matter which is easily understood if we remember how handy we have become with our elaborate modern paraphernalia for analysis. We would like to think that we have got so clever that even this unknown can be ignored. But such wishing is a weakness, human and understandable as it may be. The creative experience remains beyond every natural force powerful in affairs of moment. Let me say at once that I have no bridle for this Pegasus. I claim only

[1] An address read at the Bowdoin Institute of Philosophy, April 14, 1937.

MUTILATION NOTED
MAR 23 1960

to have seen the light of its passing a time or two; and, having seen it, to be unwilling to join further in the conspiracy to ignore it.

There is a problem here. We are forced to do the best we can. Foresight is more and more necessary if we are to guard a highly intricate technical system against the impact of unforeseen change. It is this necessity which has driven us to draw narrowing circles about the unpredictable, striving to reduce our margins of error wherever the possibility shows itself. A part of this effort is the rigorous searching of the past for the lessons it may teach; precedent behavior is expected to furnish probabilities for the future. Work of this kind has become an important university and industrial occupation, with, however, limited success. We still do not know, in any exact sense, what it means to say "other things remaining equal"; our causal chains are constantly broken by new ventures. It is true that we can forearm ourselves in certain respects and within limits; and that we have a right to certain expectations; but our forearming and our expecting have been more misleading than they would have been if we had brought creativeness into our calculations, regardless of any predisposition for completeness. We might, if we recognized it openly, learn a good deal more than we know now of its effects if not of its mechanics. While we wait for some statistical worker who will check its incidence in the past, and for a psychological study which will penetrate the mystery of its genesis, we may still discuss its general relevance to our present situation.

Mr. W. F. Ogburn once showed that invention is not wholly unpredictable if we have eyes to see. He demonstrated this by inference in an essay devoted to simultaneous discovery. It was amazing to see the number of first rank inventions which had come out of more than one mind independently. The meaning of this was that given a consistent occurrence of high intelligence in the succeeding generations of mankind, we might expect a parallel consistency in the radiating of new discoveries out of old ones. Besides those we know about formally, moreover, we can infer that there are many more of which we have never heard: little-learned but active minds must have made many of them only to find that they had already been made by others. We know also that some most significant ones were made and lost and made again until they attained such a relation to practical affairs that use could be made of them. The rise of education has ended most of this waste. The intelligence of men has, by education, been directed; its activity has been knit more closely to the past. But this means that creativeness is more a problem in the world than it ever was before;

its constant percentage in the race is brought to bear where it can be most effective—for good or for ill.

2

It seems relevant to point out yet again that the psyche belongs to individuals, and that *social* psychology is consequently a term which must be used with due reservation. To be guarded and exact it is necessary to recall that the creative experience flows in multifarious pulses through the minds of men. The mind is partly even if not wholly selective, separating out of these currents the, for it, significant materials for reflection. There are degrees of relevance in these which are difficult to predetermine. All the outer world exists or does not exist as the mind may say. The deposit of civilization is wholly within grasp. Most of it is useless, at any moment, to any individual. Yet some of it has an appeal which is irresistible.

There used to be an old puzzle which students liked to play with concerning the falling of a tree in a far forest beyond human hearing: was there sound without ears to hear? All experience presents a similar question. So far as the mind is concerned, only those materials exist which have an immediate value. Yet other materials always wait their appropriate time and place, beyond the periphery of present utility; and exigencies are apt to force their recognition simultaneously by many minds. By an approach of some such sort we can distinguish the social materials useful to a study of creativeness. They consist in the rich stores of past experience which surround and condition all we do and say; and also the accretions to this store which may result from our own activities. Creative experiences occur in the lonely travail of a single mind but they have their reasons for happening. They are unpredictable at present but they are not accidental.

The outer world is a conditioning atmosphere; it furnishes food for thought. The materials of a time, an age, are common. And creative experiences are apt, in consequence, to occur within a circle of relevance. They in turn alter the food of thought, but not usually greatly, and in a revolutionary way. The exceptions are few and widely spaced, only frequent enough to make us cautious and protective. Indeed we live among daily and conventional activity experiences with some justified sense of security in their permanence. These harden into institutions, even into vested interests, which we come to defend against the threat of change. For, in spite of their usually limited consequence, men who love security come to fear the playboy, the disrupter, the irresponsible

creative urge. The workaday world is a more or less satisfactory one; it is one at least which we have learned how to go on with. There is a powerful desire, except among the uncreative and the underprivileged, to have it stay put. This is, of course, so that it may be exploited. The creative man wants no check on his own activity; but likewise he wants no one except himself to change things. The most intelligent of men are apt, for this reason, to be the most conservative. This static state is one which he feels instinctively will not limit him; it will only keep others in their places.

But the conspiracy is seldom wholly effective. An idea may have explosive force. It is often true that the more it is confined, the greater is the effect of its release. Into the well-ordered and smoothly working system of affairs there comes one day a rumor, perhaps the merest intimation of something new. Instantly there is resentment, appeal to tradition, a cry of traitorousness; but gradually also, if the thing develops, there come accustomedness and search for probable consequence. The final result is often acceptance, even defense, and a preening of pride in accomplishment. In certain cases the smothering process is successful at some stage, and the rumor of change dies out. And there are times when genesis is stifled in another way: by catching the intelligence young and preventing its vagabond activities. Youth apparently can be kept from certain kinds of inquiry—at least if it cannot, whole nations have mistaken their policy in recent years. Our American problem is of course different. We have not told our young people not to think. We have only told them it will pay better to think how to get the best of each other. The rich soil of democracy has been soured and made less fertile in this way; but it has not been made wholly sterile. For the other method of greeting the rumor of change is to seize it at once and put it to work at making money. This is more common in our time than ever before. And it is this which makes our problem most serious. For the new idea is not only defended, it is pushed with a thousand clever devices into every cranny of modern life where it can possibly penetrate; it sets up disturbance, causes maladjustment, makes some people rich and others poor without discrimination or relation to merit. People with a sense of justice are outraged and what we call a "social problem" comes newly into view.

3

Our institutions are our daily and conventional experiences. They exist in youth, maturity and decay. Their genesis lies in the play of

creative thought upon existing habit. But the incidence of their establishment is determined by their social reception. In this sense thought is never free, functioning as it does within a scheme and order and upon contemporary material.

Frequently thought is projected backward and plays about historical material of which the mind has somehow become aware. The literary attractiveness of history, and the academic devotion to it, enable us to discover indignation concerning a manorial autocracy which once existed, or perhaps the dreadful labor conditions of the Nineteenth Century, which scholarship reveals long after the event. This in reverse may explain why so many Utopias have a reminiscent aspect. For them there are selected the most agreeable characteristics of the past; and they are surrounded with a glamour which present reality may wholly lack. But neither historical indignation nor Utopian synthesis can have much future significance. It is when thought plays upon contemporary arrangements that it may have consequences to which we must attend.

This union of present habit with vagabond thought is incidental and casual. The issue, however, is like the cuckoo in the robin's nest, a monstrosity which will not be ignored. Like mother robins, the custodians of present custom do not foresee at first the ruin about to be spread among contemporary devices. The new thing is fed and defended by those whom it will later harm, until it has grown so big and violent that it can no longer be ignored. What happens then is that the best is made of a bad job, recrimination is abandoned, and the new institution is launched upon a society which does with it whatever it must.

One of the general characteristics of the present economy which is often overlooked is that the balance is badly kept. Encouragement has been given much more generously to profit-making devices than to others which might have tempered their effects. The monsters of industrial technique have, for a century now, not only been encouraged to lay their eggs where they liked, but their offspring have been cherished and protected. Mankind is by way of being crowded out of its nest by that unnatural hybrid, the machine.

It was to this general problem that Veblen addressed himself when he reversed the traditional saying that necessity is the mother of invention. "This," he said, "appears to be nothing better than a fragment of uncritical rationalism." His theory was that invention determined necessity. This rather startling thesis was advanced in his

Instinct of Workmanship. In *The Theory of the Leisure Class* his implications were somewhat clearer, especially with respect to the relation between creativeness and institutions; "Whatever approves itself to us on any ground at the outset presently comes to appeal to us as a gratifying thing in itself, it comes to rest in our habits of thought as substantially right."

This *tour de force* of reversal is doubtless a just comment on the nature of cultural change. Perhaps this adage is no truer than many another old wives' saying; it nevertheless has an element of truth which he overlooked in his desire to convict its purveyors of reflecting, as he said, "an ancient preconception by help of which the spokesmen of edification were enabled to interpret all change as an improvement due to the achievement of some definitely foreknown end." He certainly qualified when he went on to say, "Doubtless the felt need of ways and means has brought on many changes in technology," a point which is extremely important to one who would assess in any way the consequences of creative acts. The felt need of ways and means is stimulus and condition for invention. It may be true that "the ulterior consequences of any one of the greater mechanical inventions have been neither foreseen nor intended in the designing of them," but that does not affect the point at issue here. Perhaps they could not be foreseen; perhaps the stimulus which stirred the impulse was indifferent to social consequences or bent on personal gain. But the individual worked in creative fashion; and the social consequences were significant; and neither was entirely outside foreseeable possibility. Within limits we may know what inventions and discoveries are likely; within limits we can prepare for the consequences. At least we need not accept the inference that in a democracy certain creative activities are or can be stopped (especially when we are actively engaged in encouraging many of them) and that their results will not require accommodation.

The thesis here is that we need a policy with respect to creativeness. The outlines of that policy must, however, grow out of considerations which have to do with its utility. We may smother it if we are satisfied with things as they are now; or we may direct it by judicious encouragement. There are no other alternatives. It can never be wholly free and undirected as is sometimes casually suggested. We should cease to be a society when that could happen. But the first alternative, though usually accepted, does not stand up well under examination either. Complete repression would be possible only among robots. What is meant by policy therefore is that we may tentatively agree on

what we shall encourage and what we shall not. We both encourage and repress now; but because we have never projected alternate results we do it badly. We have fortuitous emphases, ones which some one or some group wants us to have; and they are frequently in conflict, sometimes self-cancelling, frequently against the general good. There is an unnoticed force here which can transform the world—indeed it will transform the world. It is suggested that we ask: How?

4

If our institutions are our activity experiences, which is a statement often used for definition, the theoretical position is exactly analogous to the behaviorist one. If the behaviorist would say that men are what they do, the institutionalist would say that society is as it acts. Behaviorism throws over the paraphernalia of consciousness; institutionalism has no use for it either in its other forms—group consciousness, for instance. The notion of a sort of supermind has always been attractive. So keen a thinker as Pierce made an excursion into this far country, which shows at least that there exists an empty valley of differentiated group behavior and that there is a constant temptation for thought to flow into and fill it. For thought, no less than nature, abhors a vacuum. But the analogy always depended on an original concept of individual consciousness and those who accept behaviorism in this field, as a working principle, ought reasonably to accept it in their analysis of group action.

It is worth remarking, however, that this remains, as it always was, an analogical *tour de force*, and, as such, possesses dangers. For it is relatively easy to convict some institutionalists of harboring a rather stubborn paradox. We do not get far by saying that institutions are activity experiences and then reversing the formula to say that activity experiences are institutions. If our institutions determine our attitudes and our tasks—if they are, in fact, the food of our thought—then it is difficult to account for change. Still, if any one characteristic is more prominent than another in our society it is that change is constant. Even the classical economist had to take account of its disturbing force by postulating static states; and it has been true of other sciences that despair has driven workers in them to adopt some variety of the one-thing-at-a-time method. These may have been unsuccessful attempts to confine natural and social processes in straight-jackets of limited working apparatus but their prevalence points to the ubiquitousness as well as the unruly nature of change. Formidable attempts of statisticians to reduce social processes to number, line and trend have failed

for the same reason. There is misbehavior everywhere in our naughty world.

The fact is, of course, that institutions are only partially responsible for what we do and think. And even if we allow their determinateness wide scope, the influence may run to active rejection as well as to active defense. Unrest is about as characteristic of human nature as is acceptance. Unrest, too, like acceptance, has an irregular incidence. There are likes and dislikes in all of us mixed in dissimilar patterns. Small causes sometimes have large if distant effects, a lesson we might have learned from the Freudians if it had not been learned before. And besides all this there is a racial pulse which runs toward rather than from the world. Some actions are not selective. They begin within the glands and to a point are independent of environment.

Evidently definition has to be more exact. We cannot identify absolutely "activity experience" and "institution." We have to qualify and say that institutions are those activity experiences which have become habitual. For those small causes with far effects, of which mention was just made, were also activity experiences; and so, for that matter, is the most passionate and unreflective of creative acts. The question becomes at last one of those absurd and unfruitful ones over which we like to puzzle but which get us nowhere. What it means, really, to say that institutions are activity experiences become habitual is that some of these experiences attain social importance and that some do not. The ones which do, affect more people in more ways. But even the ones which do not—or whose growth is delayed or prevented—may, like certain famous dissenting opinions of Supreme Court Justices, have more to commend them to the ordinary judgment than those which do. The practical task which confronts us is that of explaining a process of social selection, not that of accounting for origins. It is just as well, at present, to leave origins alone. The mysteries here are quite as deep as those which have to do with personal attraction; the two, indeed, seem to have many similarities. And the same rough wisdom of accountability applies almost equally. Propinquity has something to do with it; so has prudence, diet, and, perhaps, the weather. These questions will not be let alone; and something may come of them. But about all this we have much to learn before we shall be able to circle the conceiving of institutions with controls.

5

One thing which needs to be more consistently recognized is that social control is in itself an activity experience; and that inventiveness

functions here as elsewhere, ceaselessly and unpredictably. This is a time when, out of all reason, and in spite of little reward, that kind of inventiveness nearly matches the profit-making kind. These activities are going on all about us—that is to say new instruments for this purpose are being suggested. We can see necessity playing her ancient rôle as the mother of invention. The mortality is high; but this is because the climate is still unfavorable, and because thousands of natural enemies are seeking to destroy. Unfortunately the dominant classes of our time regard these youngsters as potential enemies and, like the old bulls of a buffalo herd, seek untiringly to drive them out into the plains where they must perish without progeny. And, equally unfortunately, we have no welcoming committee for genuinely benign discoveries.

It is nevertheless a mistake to think that control can be driven out of any society by killing off the ideas of all those who would have it exercised in the group interest rather than in that of the old bulls. Management always exists. The only question is: in whose interest? There has been a long attempt to escape the acceptance of this and to prevent the growing generation from seeing that it is so. Our contemporary theory of *laissez faire* is a rationalization which apparently convinces many of us that mere suppression of governmental interference with business keeps all control out of society. It does nothing of the kind, of course; it merely centers it in other places. The difficulty is that management exercised in pursuit of individual or even group objectives often runs at cross purposes with controls which may be proposed in pursuit of wider social aims. Not all the instruments suggested for control depend on the governmental system for their validity; but those which do not seldom have more to recommend them than that they would be beneficial to some group which is competing with business for the social surplus, and even though some who do not belong to that group would grant it concessions, (farmers, workers, for instance), out of a feeling of justice, even these remain unconvinced that permission to exploit the community is safe in such hands beyond a certain point. It is because government is the only organ still remaining to us which infuses all interests and can attain comprehensive purposes that it is returned to again and again as the most practical instrument. Government still is a center; even its emasculated functions, under the conventions of industrial *laissez faire*, still touch, in one way or another, every phase of life; and suggestions for the use of any other agency can be traced to a remainder of faith in the principle of competitive struggle.

The influence of evolutionary theory has doubtless much to its credit; but our faith in survival through struggle, and fitness because of survival, is certainly not one of the credit items. It is this belief which is perhaps the strongest bulwark of irresponsible control. We never seem to learn that, among social groups, prosperity proves nothing except the existence of might; and that such success may be earned at a terrific expense to others.

There is the consideration, also, that *laissez faire* has always functioned better as a defense myth than as the automatic regulator which Adam Smith liked to postulate. The ease with which its assumed opposite—Communism—was erected into a *bête noire* shows this; it is talked of as though it were the only alternative to no control at all. Meanwhile, the more sophisticated use the great powers of government freely to advance their own designs. Liberty, freedom, and democracy have degenerated into catch-words with emotion-invoking powers but without substantial content. Those boys who talk most loudly about elementary morality can usually be discovered at some favorable moment slipping furtive hands into the family cookie jar. The astounding thing about the bad boys of our adolescent industry is the success with which they talk us out of either punishing them, locking up the cookies, or setting up rigorous rules to assure common access, but no favoritism, in sharing whatever there may be.

6

Belief in a saving selection through struggle is natural to a society in which large areas of life have already been organized on the struggle theory. It is not so much mistaken to view this process as one which actually takes place, as it is to disregard alternatives, as we are led to do by accepting the ethical aura which has been thrown around the word survival. This characteristic justification of competition as a salutary principle has, however, led to a certain overemphasis in description which raises suspicion among critical observers. One who wears the glasses prescribed by nineteenth century orthodoxy is unable to see at all many phenomena which, when they are seen with an impartial eye, assume very different aspects. Competition exists, but so does its opposite; and, with surprising persistence, variously named cooperative movements continue to function in spite of an inimical climate and the frowns of the pundits.

Indeed it is possible to adopt a view of the past few centuries of thought which make it seem that dividing lines were frequently very

narrow, that what was to become orthodox and what dissent was often undecided. Our particular span of days is, of course, lived in the backwash of a vast creative explosion in the waters of theory. Darwinism drowned dissent in short order. But Darwin was also regrettably saddled *ex post facto* with Spencer. It is rather interesting to note, as a comment on human weakness for analogy, (which is shared by more of us than just those who belong to the legal fraternity), that the fragment of Darwinism first to lodge in economics and to become encysted there, was the notion of struggle and the survival through it of the best agencies for economic tasks. For this theory there was a pleasant home all prepared. Indeed, as everyone now knows, it was Malthus, a clergyman-economist, who had something definitive to do with the genesis of the idea.

This creative explosion in the nineteenth century is precisely one of those unpredictable occurrences which have far and unforseen results. One would not have said off-hand that this was a particularly Christian doctrine; yet, at more than half a century removed, a most Christian President of the United States argued that the preservation of individualism, competition and struggle were the exclusive roads to mercy, justice and the elevation of the general welfare. This transmutation ought to be a lesson not only in the dangers of analogical logic, but also in the penetrative power of ideas. It is idle to argue that creative ideas are inconsequential, that they are merely after-the-fact explanatory gestures, arising from a human weakness for bringing order out of chaos. They are much more than that.

It is quite possible to reverse the meaning of this illustration and to show that the consequences of this particular idea prove, if they prove anything, that evolution by struggle is not an inevitable process nor even a wholly prevalent one. It might not even be too much to insist evolution takes place in quite another way. Assume, for the moment, what there is ample evidence for, that generative social acts which set important consequences in train consist frequently in such creative explosions as Darwin let loose in the world; and that chain-causation describes the accretion of results as the idea spreads through human minds like heavy coal-smoke in a windless valley. There is no struggle here of the competitive sort, at least as we usually mean it. There is, of course, that fearful, long, and lonely labor of the mind, throughout the mysterious process by which ideas come to birth, (which is so inadequately described in any present analysis), and there is the entrance into the world of the idea and its growth or early death. But this has nothing to do with the theory of social change which depends on struggle

among social groups. This last depends on a false concept of complete identification among individuals to form groups in the first place; and in the second place, on an unreal picture of herds locking horns over limited pastures which is borrowed from the primitive fringes of social life.

We have so little need, really, for the notion of struggle as a principle of social change, that we might forthwith abandon it with few losses and many gains. The great gain, however, would be not in changing our view of the past, which is of less importance than most of us would like to think, but in reconstructing our view of the future. Struggle has become too ready a formula. It leads us too easily to saying that natural competition among the elements of the present will yield the dominant forces of the future. The results we get are as specious as the premise. If we consider creation at all, we think absurdly of a competitive struggle among discoveries to be born. It is true that we can consign them, once they are born, to the doubtful mercies of competition. We can continue to set up struggle and enforce it; we can require ideas to find their feet unhelped in a hostile world. But that is a kind of social suicide which is not unlike the crude notion of physical selection which held that the fittest infants would survive unaided the attacks of infantile disease. A very little thinking demonstrated that no useful fitness was sacrificed by removing children from exposure to small-pox and diphtheria. There was no gain from breeding for resistance to attacks of this sort. There were more important qualities, however illy they may have been defined. Similarly new ideas concerning social management may have a validity which is not to be tested by survival in a war-torn economic system. Those which can exist are predetermined by the qualities we favor.

So, as we may see, a perfectly good creative act may be killed in infancy simply through lack of protection from accident. Or it may be exposed, as the Greeks exposed their unwanted babies on barren hillsides. Parental love does something toward the guarding of the race; but parents sometimes object to the vaccinative activities sponsored by the social group. It is largely a matter of what it is we value. And any society which assumes the attitude that brain-children ought at once to be subjected to unrestrained competition, not only with other infants, but with all the jealous adults of the community too, shows quite clearly where its values lie. It wants, above all, to protect existing things, forces, privileges. It wants no change except the sort which will pile new privileges on the old ones. And for the same people.

I was discussing, the other day, some of the expeditions of modern

thinkers into the mysterious hinterlands of religion. I ventured the notion that the inner strength on which the Cromwellian ironsides could march when food was lacking was best understood as a true creative experience. The conditions were prepared in which many men created God; in the process other wonders were added unto them. I thought it had usually been so, and that the great enemy of religious experience is old dogma, just as it is the great enemy of discovery, preventing men, as it does, from achieving this experience of creation in various fields of science. My friend was aghast at the casual accusation that scientists could be governed by preconception. Yet, after thinking it over, he admitted that there are many wearers of scientific hoods who feel very strongly their membership in a kind of new order whose orthodoxies are best defended by inquisition. This seemed most true too in those fields where laws are largely borrowed and where experiment is especially difficult and dangerous.

The social sciences are meant, of course. The quick objections my friend made to the joining of "science" and "dogma" gave me pause. How, I thought, had I come to join the two or how had the juxtaposition become comfortable in my mind when his had rejected it? Then I realized that for a long time I had been growing more and more impatient with the neglect of creativeness. The humility of the old scientists seemed to have been lost and its place to have been taken by arrogance among contemporaries. Any thinking which is not scientific is not counted as thinking at all. But the procedures of science as such, if I understand them, are negative rules whose rigidity is intended to prevent error. Scientists never do anything which is more than mildly ingenious as scientists. They check, test, and make certain, but nothing more. No separation could be clearer than that which exists between invention and experiment; he who has no power to generate ideas is a routinist. Examination of the accepted lists of great names in science will show first of all that they were explorers in unknown lands. Often enough the experimentation was done afterward by others.[2]

7

The formation of a policy concerning creativeness is not a simple matter for government, foundation or university. What each can do is

[2] There is a discussion of the meaning of method in Mr. Stuart A. Rice's introduction to "Methods in Social Science," in which he refers to the difficulty of discovering how hypotheses originate in the minds of their discoverers, and to the desire of methodical theorists to exclude this from consideration.

limited and permissive, as to the creative process itself. Leisure for genius ought to be relatively easy to arrange and very little else is necessary to assure the continuance of productivity. But what kind of productivity? And what kind of use shall be made of the product: These are the questions which have social rather than individual implications. We have been letting them go by default. But this does not amount to keeping hands off. It amounts to letting certain interests put hands on. What the results of this can be we have had amply demonstrated in the last few years.

The furnishing of a climate favorable exclusively to technological discovery and to its immediate wide-spread adoption has left us with an incomparable power to make goods without the power to distribute them. The consequence in depression, unemployment and misery ought to have taught us reform. But there are few signs that it has. So basic a matter as the dissemination and control of discovery is quite overlooked in our renewed faith in making money at each others' expense. There are impending or in beginning use today new processes, machines, materials and ideas which will, if their consequences remain uncontrolled, produce chaos some years hence.

This is one problem. Another has to do with the economists' insensibility to the impact of creativeness. There is something about the rigidity of theory or the neatness of figures which makes it peculiarly difficult for the inevitable incidence of change to be admitted for consideration. The phantom "X" remains, and for all we know now, will remain an unpredictable quantity. But there need not be a pretense that this is not so. And there need not be the contemporary resistance to the admission of evidence that change is impending, even though its results, lacking control, are difficult to assess.

One side of mankind's history is a story of struggle under a load of work and suffering; another side is a story of brilliant adventures in creativeness. On the whole these achievements have lightened the miseries of poverty. But sometimes they have not. We have reached a stage of technology at which the likelihood of good results from inventiveness is almost wholly dependent upon social control. If we refuse to extend control and continue to rely on self-interested exploitation of new discoveries we shall suffer the penalty we deserve. Creativeness is inevitable; its irresponsible loosing upon an unready world is not. We can depend upon it in our march toward the stars, but only if it can be harnessed to a chariot which is going to be driven that way. Discovery only creates change, not progress. Progress is something which

men, not man, are responsible for. And men rather than man will have to insure it. Perhaps we may create that too, many of us at once, as the Cromwellian armies created a special God which sustained them better than food. We have an adequate reason. Our campaign for the conquest of poverty, disease and insecurity is a true crusade, lying deep among racial aspirations, and therefore close to the creative talents. Furthermore there is a clue here to the way in which man may be merged again in mankind and a unity of purpose achieved which will dispel our bickerings. We might thus be set upon our way with a music more useful than the sweet, betraying rhythms of the drums to which men in competitive struggle have always marched. The rhythms would be less primitive and, in that sense, stirring; but they would fashion a symphonic whole of purpose and achievement, in which creativeness, the highest ability and the most satisfying experience of man, might play the benign part for which it was shaped by nature.

120 Wall St., New York.

Vagueness

An exercise in logical analysis

BY

MAX BLACK

"Vagueness and accuracy are important notions, which it is very necessary to understand." (Bertrand Russell).

"The notation, however, is what we lack, and the verdict of the mere feeling is liable to fluctuate." (Henry James).

1. INTRODUCTION

IT IS a paradox, whose importance familiarity fails to diminish, that the most highly developed and useful scientific theories are ostensibly expressed in terms of objects never encountered in experience. The line traced by a draughtsman, no matter how accurate, is seen beneath the microscope as a kind of corrugated trench, far removed from the ideal line of pure geometry. And the "point-planet" of astronomy, the "perfect gas" of thermodynamics, or the "pure species" of genetics are equally remote from exact realization. Indeed the unintelligibility at the atomic or sub-atomic level of the notion of a rigidly demarcated boundary shows that such objects not merely are not but could not be encountered. While the mathematician constructs a theory in terms of "perfect" objects, the experimental scientist observes objects of which the properties demanded by theory are and can, in the very nature of measurement, be only approximately true. As Duhem remarks, mathematical deduction is not useful to the physicist if interpreted rigorously. It is necessary to know that its validity is unaltered when the premise and conclusion are only "approximately true."[1] But the indeterminacy thus introduced, it is

[1] P. Duhem: "... une deduction mathématique n'est pas utile au physicien tant qu'elle se borne à affirmer que telle proposition, rigoureusement vraie, a pour conséquence l'exactitude de telle autre proposition. Pour être utile au physicien, il lui faut encore prouver que la seconde proposition rest *à peu près* exacte lorsque la première est seulement *à peu près* vraie" (*La Théorie Physique*, p. 231).

necessary to add in criticism, will invalidate the deduction unless the permissible limits of variation are specified. To do so, however, replaces the original mathematical deduction by a more complicated mathematical theory in respect of whose interpretation the same problem arises, and whose exact nature is in any case unknown.

This lack of exact correlation between a scientific theory and its empirical interpretation can be blamed either upon the world or upon the theory. We can regard the shape of an orange or a tennis ball as imperfect copies of an ideal form of which perfect knowledge is to be had in pure geometry or we can regard the geometry of spheres as a simplified and imperfect version of the spatial relations between the members of a certain class of physical objects.[2] On either view there remains a gap between scientific theory and its application, which ought to be, but is not, bridged. To say that all language (symbolism, or thought) is vague is a favorite method for evading the problems involved and lack of analysis has the disadvantage of tempting even the most eminent thinkers into the appearance of absurdity. Duhem claims that "for the strict logician," a physical law is neither true nor false.[3] For Einstein mathematics is either uncertain or inapplicable,[4] and Russell cheerfully sacrifices logic as well.[5]

The aim of this paper is to avoid such wholesale destruction of the formal sciences by supplying in greater detail than has hitherto been attempted an analysis and symbolism for the "vagueness" or "lack of precision" of a language.

[2] Plato: "Those who study geometry and calculation . . . use the visible squares and figures, and make their arguments about them, though they are not thinking about them, but about those things of which the visible are images. Their arguments concern the real square and a real diagonal, not the diagonal which they draw, and so with everything. The actual things which they model and draw . . . they now use as images in their turn, seeking to see those very realities which cannot be seen except by the understanding." (*Republic*, 510—Lindsay's translation.)

[3] "Toute loi physique est une loi approchée; par conséquent, pour le strict logician, elle ne peut être, ni vraie, ni fausse." (Loc. cit. p. 280.)

[4] "Insofern sich die Sätze der Mathematik auf die Wirklichkeit beziehen sind sie nicht sicher, und insofern sie sicher sind, beziehen Sie sich nicht auf die Wirklichkeit." (*Geometrie und Erfahrung, p. 3.*)

[5] "All traditional logic habitually assumes that precise symbols are being employed. It is therefore not applicable to this terrestrial life, but only to an imagined celestial existence." (*Vagueness, Australasian Journal of Philosophy*, Vol. 1 (1923), p. 88.)

In this paper Russell contends that "all language is more or less vague." Again the "laws of Excluded Middle is true when precise symbols are employed but it is not true when symbols are vague, as, in fact, all symbols are." (Ibid. p. 85.)

We shall not assume that "laws" of logic or mathematics prescribe modes of existence to which intelligible discourse must necessarily conform. It will be argued, on the contrary, that deviations from the logical or mathematical standards of precision are all pervasive in symbolism; that to label them as subjective aberrations sets an impassable gulf between formal laws and experience and leaves the *usefulness* of the formal sciences an insoluble mystery. And it is the purpose of the constructive part of the paper to indicate in outline an appropriate symbolism for vagueness by means of which deviations from a standard can be absorbed by a re-interpretation of the same standards in such a way that the laws of logic in their usual absolutistic interpretation appear as a point of departure for more elaborate laws of which they now appear as special or limiting cases. The method yields a process by which deviations, when recognized as such, can be absorbed into the formal system. At every stage the mathematics we already employ will provide the material for the increasing accuracy of the next stage.

It is one of the paper's main contentions that with the provision of an adequate symbolism the need is removed for regarding vagueness as a defect of language. The ideal standard of precision which those have in mind who use vagueness as a term of reproach, when it is not a shifting standard of a relatively less vague symbol, is the standard of scientific precision. But the indeterminacy which is characteristic of vagueness is present also in all scientific measurement. "There is no experimental method of assigning numerals in a manner which is free from error. If we limit ourselves strictly to experimental facts we recognize that there is no such thing as true measurement, and therefore no such thing as an error involved in a departure from it."[6] Vagueness is a feature of scientific as of other discourse.

The impressionist painting of a London street in a fog is not a vague representation of what the artist sees, since his skill largely consists in the accuracy with which the visual impression is transcribed. But the picture is called vague in relation to a hypothetical laboratory record of the wave lengths and positions of the various objects in the street, while it is forgotten that that record, in supplying additional detail, obliterates just those large scale relations in which the artist or another observer may be interested. This paper is written to show that while the vague symbol has a part to play in language which cannot be equally well performed by more accurate symbols from another level (wave

[6] N. R. Campbell, *Measurement and Calculation*, p. 131.

lengths as a substitute for names of colors) the transition to levels of higher accuracy can always in principle be made.

2. SUMMARY OF THE PAPER'S ARGUMENT

The process of logical analysis of a language can be regarded as the exhibition of a set of conventions for the use of symbols, abstracted from the regularity of linguistic habits in some postulated speech community, and proceeding by a series of successive approximations involving the use of "simplified" or "model" entities.

The vagueness of symbols in any such abstract system is a symptom of the degree of deviation of the "model" language from the empirically discoverable linguistic habits in the corresponding speech community.

A typical example of vagueness is described. A symbol's vagueness is held to consist in the existence of objects concerning which it is intrinsically impossible to say either that the symbol in question does, or does not, apply. The set of all objects about which a decision as to the symbol's application is intrinsically impossible is defined as the "fringe" of the symbol's field of application. It is claimed that all symbols whose application involves the recognition of sensible qualities are vague, and a typical case is constructed for convenience of reference. Vagueness is distinguished from generality and from ambiguity. The former is constituted by the application of a symbol to a multiplicity of objects in the field of reference, the latter by the association of a finite number of alternative meanings having the same phonetic form; but it is characteristic of the vague symbol that there are no alternative symbols in the language, and its vagueness is a feature of the boundary of its extension, and is not constituted by the extension itself. Russell's definition of vagueness (in a paper to which frequent reference is made) as constituted by a one-many relation between symbolizing and symbolized systems is held to confuse vagueness with generality.

The assumption of the existence of a well-defined set of objects to which the application of a vague symbol is doubtful is shown to be inconsistent with the usual meaning of negation, and the conclusion is shown to follow whether the number of individuals in the field of reference is finite or infinite. But it is shown that there is no good reason to regard the defining characteristic of vagueness as "subjective." The crude notion of the "fringe" is therefore replaced by a statistical analysis of the frequency of deviations from strict uniformity by the "users" of a vague symbol. In this, the most important section of the paper it is found possible to define the notion of a consistency profile

or, its equivalent, a consistency function, corresponding to each vague symbol and thus to classify, or even, theoretically, to measure, degrees of vagueness.

The definition of consistency profile is based on the existence of "a group of users of a language" whose linguistic habits are sufficiently stable and inter-correlated to permit of limiting assertions concerning frequencies of deviations from a standard. The definition of the users of a symbol is shown to be another aspect of the definition of the symbol itself, and the relation between the two processes is illustrated by analogy with the definition of a biological species. An experiment is described whose results illustrate the construction of a consistency profile, and the analysis is extended to the consideration of logical relations between vague symbols.

3. VAGUENESS DESCRIBED

The vagueness of a term is shown by producing "borderline cases," i.e., individuals to which it seems impossible either to apply or not to apply the term. Thus a word's vagueness is usually indicated, more or less explicitly, by some statement that situations are conceivable in which its application is "doubtful" or "ill-defined," in which "nobody would know how to use it" or in which it is "impossible" either to assert or deny its application.

Peirce's definition[7] is admirably clear: "a proposition[8] is vague when there are possible states of things concerning which it is *intrinsically uncertain* whether, had they been contemplated by the speaker, he would have regarded them as excluded or allowed by the proposition. By intrinsically uncertain we mean not uncertain in consequence of any ignorance of the interpreter, but because the speaker's habits of language were indeterminate."[9] An example will now be discussed in more detail.

[7] Baldwin's *Dictionary of Philosophy and Psychology*, II, 748.

[8] In this paper reference will always be made to the vagueness of a word or symbol, but no important difference is involved in speaking of a proposition's vagueness. The proposition can be regarded as a complex symbol and its vagueness defined in terms of that of its constituents, or vice versa.

[9] In the remainder of the passage Peirce explains that by an indeterminacy of habits he means the hypothetical variation by the speaker in the application of the proposition, "so that one day he would regard the proposition as excluding, another as admitting, those states of things." But the knowledge of such variation could only be "*deduced* from a perfect knowledge of his state of mind; for it is precisely because these questions never did, or did not frequently, present themselves, that his habit remained indeterminate."

Let us consider the word *chair*, say. On reflection, one is impressed by the extraordinary variety of objects to which the same name is applied: "... think of arm chairs and reading chairs and dining-room chairs, and kitchen chairs, chairs that pass into benches, chairs that cross the boundary and become settees, dentist's chairs, thrones, opera stalls, seats of all sorts, those miraculous fungoid growths that cumber the floor of the arts and crafts exhibitions, and you will perceive what a lax bundle in fact is this simple straightforward term. In co-operation with an intelligent joiner I would undertake to defeat any definition of chair or chairishness that you gave me."[10]

It is important in such a case that the variety of application to objects differing in size, shape and material should not be confused with the vagueness of the word. The variety of application no doubt arises from the fact that chairs are defined by the need to be satisfied. "... Every common noun, every concept is essentially merely an affective grouping. In a plurality of objects, differing from the point of view of perception even very widely from one another, we discover the same capacity to satisfy some given affectivity, some given need or desire of ours, and through this capacity we reduce this very plurality to a unity."[11] Being "a separate seat for one," as the dictionary puts it, is compatible with much variation in form and material.

But in speaking of the vagueness of the word chair, attention is directed only to the fact that objects can be presented whose membership of the class of chairs is incurably "uncertain" or "doubtful." It is the indeterminacy of the usage, not its extension, which is important for the purpose of the argument. The finite area of the field of application of the word is a sign of its *generality*, while its vagueness is indicated by the finite area and lack of specification of its boundary.[12] It is because *small* variations in character are unimportant to success in serving

[10] H. G. Wells, *First and Last Things*, p. 16.

[11] E. Rignano, *Psychology of Reasoning*, p. 109.

[12] Cf. B. A. W. Russell "A vague word is not to be identified with a general word" (*Analysis of Mind*, p. 184). He adds, however, "that in practice the distinction is apt to be blurred" and blurs it himself in saying "a memory is vague when it is appropriate to *many* occurrences" (*Loc. cit.* p. 182). This confusion between generality and vagueness invalidates his neat definition "the fact that meaning is a one-many relation is the precise statement of the fact that all language is more or less vague." (*Vagueness*, p. 89.)

The confusion may ultimately be traced to a certain uneasy nominalism in Russell's philosophy which tends to treat generality and vagueness indifferently as imperfections of symbolism in relation to the attempt to describe a universe composed exclusively of absolutely specific or atomic facts.

the purpose of being "a separate seat for one" that it is possible, by successive small variations in any respect, ultimately to produce "borderline cases." The cumulative action of such variation in producing large additive effects is at the root of the felt inability either to withhold or to apply a general term to the unusual and the extreme case.

One can imagine an exhibition in some unlikely museum of applied logic of a series of "chairs" differing in quality by least noticeable amounts.[13] At one end of a long line, containing perhaps thousands of exhibits, might be a Chippendale chair: at the other, a small nondescript lump of wood. Any "normal"[14] observer inspecting the series finds extreme difficulty in "drawing the line" between chair and not-chair. Indeed the demand to perform this operation is felt to be inappropriate *in principle*: "*chair* is not the kind of word which admits of this sharp distinction" is the kind of reply which is made "and if it were, if we were forbidden to use it for any object which varied in the slightest way from the limiting term, it would not be as useful to us as it is." This is the sensible attitude but it raises difficulties for logic.

In order to circumvent these difficulties, we shall make use of the fact that the uncertainty of a single normal observer, or the variation in the decisions made by a number of such observers, either of which can be taken as the definition of vagueness, is a matter of degree, varying quantitatively, though not regularly, with the position of an object in the series. At the extremities of the series little or no uncertainty is felt, but the observer grows increasingly doubtful when the borderline cases in the center are approached; "everybody" agrees that the Chippendale chair *is* a chair, "nobody" wants to sit upon, still less to call a chair, a shapeless lump of wood, but in intermediate cases personal uncertainty is a reflection of objective lack of agreement.

We have used alternative but correlated definitions of vagueness in order not to prejudge the issue whether vagueness is subjective or objective.[15] On the one hand we can use an observer's feelings or report of his feelings; on the other, the set of divisions made by a set of inde-

[13] The variation of this amount with the choice of the observer, and with conditions affecting the same observer, strengthens the subsequent argument by introducing further indeterminacy into the operation of "drawing the line."

[14] This is, in part, a definition of the "normal" observer; we shall reject the testimony of an observer who claimed to have discovered *the* point at which the division was to be made. Cf. section 7 for a fuller discussion of this point.

[15] See section 5, below.

pendent observers who are given sufficient inducement to make a unique division in the series irrespective of their feelings of uncertainty.[16]

The vagueness of the word *chair* is typical of all terms whose application involves the use of the senses. In all such cases "borderline cases" or "doubtful objects" are easily found to which we are unable to say either that the class name does or does not apply. The case of a color name, whose relative simplicity is unobscured by the variation in application of such "artificial" names as chairs, is specially striking. If a series of colored cards of uniform saturation and intensity are arranged according to shades ranging by least perceptible differences from reds through oranges to yellows, the "uncertainty" which is typical of vagueness is at once demonstrated. "The changes of color in the spectrum are throughout so continuous that *it is not possible to find the exact point at which the changes of direction begin.*"[17] It would be easy, but uninstructive, to multiply examples. Reserving the terms of logic and mathematics for separate consideration,[18] we can say that all "material" terms, all whose application requires the recognition of the presence of sensible qualities, are vague in the sense described.

4. LOCATION OF THE FRINGE

The quantitative variation in the degree of uncertainty felt by a typical observer, or the equivalent variation in the divisions made by a number of observers, will be used later as the basis of a method for symbolizing vagueness. But before doing this it is necessary to dispose of a plausible but mistaken view which seeks to solve the problem of border line cases by allocating them to a region of "doubtful application," a kind of no man's land lying between the regions when a term applies and does not apply. For even if it is granted that all material terms are vague in the sense described it might still be said that the existence of border line cases is unimportant. Such cases occur so infrequently, it might be argued, that consideration can always be restricted to objects concerning which the "doubt" does not arise. To such objects difficulties concerning the indeterminacy of the boundary will

[16] Cf. the experiment described in Appendix I when the subject agrees beforehand to make a unique division, the "inducement" being desire to keep his word, or curiosity, or some other motive.

[17] Stout, *Manual of Psychology*, p. 160. This manner of phrasing the situation suggest of course that the fault is in the language or in imperfect perception: there *is* an "exact point" where the transition occurs but we are unable to find it.

[18] See Appendix II.

not be relevant; for these cases will remain unproblematic whichever separation is made in the field of application, and since we do not choose to argue about borderline cases no difficulty remains.

An objection of this sort misses the point: we do not claim to have discovered a serious practical difficulty, but are trying to achieve the accomodation of an unduly simple conception of logic to the undoubted practical efficacy of formally invalid classificatory procedure. The presupposition of the existence of a class of "doubtful" objects will involve the assumption either of an exact boundary or of a doubtful region (of the second order)[19] between the fringe and the class of unproblematic objects.

Either assumption will be shown to be incompatible with the usual definition of negation, and thus indirectly incompatible with the strict application of logical principles. The exposition will be simplified by using a set of constant symbols to illustrate the features of vagueness described in the last section. Let L then, be a typical example of a vague symbol. It has been seen that the vagueness of L consists in the impossibility of applying L to certain numbers of a series. Let the series S, say, be linear, and composed of a finite number,[20] ten say, of terms x, let the rank of each term in the series be used as its name (so that the constant values of the variable x are the integers one to ten inclusive). Finally let the region of "doubtful application" or "fringe" be supposed to consist of the terms whose numbers are five and six respectively. There is, of course, no special significance in the choice of the numbers, ten, five, six, which are taken simply for the convenience of having definite numbers to which to refer.

In the usual notation of the propositional calculus Lx will mean *L applies to X* and ~Lx will mean *L does not apply to x* or *Lx is false*. (synonymous expressions)

Suppose now that L1, L2, L3, L4 are true, while L5 and L6 are "doubtful." It can only follow that to assert Lx of any x is positively to exclude it only from the range 7 to 10, since we cannot be sure, when Lx is asserted, that x does not perhaps occur in the range 5, 6. Thus to assert Lx is tantamount to confining x to the range 1 to 6.

Having obtained this result, it is easy to construct a similar argument in respect of ~Lx. The assertion of ~Lx can, no more than the assertion of Lx, positively exclude x from the fringe 5, 6. It follows that to

[19] Russell assumes an infinite series of doubtful regions, each fringe having a fringe of higher order at its boundary, but does not pursue the consequences of this assumption.

[20] The hypothesis of an infinite series is considered later in this section.

assert ∼Lx is tantamount to excluding x from the range 1 to 4 and confining it to the range 5 to 10.

In short, inability to find a logical interpretation of doubtful and perhaps in terms of the two truth values, truth and falsehood, forces us to admit that the ranges of application of Lx, 1 to 6, and of ∼Lx, 5 to 10, overlap in the fringe, 5, 6.

On the other hand, the statement ∼Lx is, by definition of the logical operation of negation, true only when Lx is false, and false only when Lx is true. If, as we have assumed, asserting Lx confines x to the range 1 to 6, Lx is false only when x belongs to the range 7 to 10. Thus in contradiction to our previous result that ∼Lx is true when and only when x belong to the range 5 to 10, ∼Lx should be true when and only when x belongs to the range 7 to 10. The formal properties of logical negation are incompatible with an interpretation which allows the domain and the complementary domain of a propositional funtion to overlap.

We can clinch the argument by attempting to translate the definition of L's vagueness, in some such form as *there is at least one term to which neither L nor its contradictory applies*, into the symbolism of the propositional calculus. Translating the italicized phrase in the last sentence gives

$$(\exists x)\ \{\sim L(x)\cdot\sim(\sim L(x))\}$$

which is at once transformed, by the rule of double negation, into

$$(\exists x)\ \{\sim L(x)\cdot L(x)\}$$

which is a contradiction. Such a contradiction is only to be evaded by denying the equivalence of ∼(∼L(x)) and L(x), i.e. by refusing to identify the operation ∼ when prefixed to a vague symbol with the ordinary operation of negation. This point of view will be incomplete and unplausible unless it is possible to define the new sense of ∼, i.e. to give the rules according to which the sign is to be used.

The situation is in some ways comparable to the re-interpretation of negation in mathematics arising from the criticisms of the Intuitionist school of philosophers of mathematics. Refusal to accept a theorem and its contradictory as exclusive alternatives forces the Intuitionists to construct a logical calculus, in which ordinary negation is eliminated, its function being taken by a new notion differing in its formal properties.[21] Whereas, however, the Intuitionists, setting out from a fairly

[21] Cf. M. Black, "The Claims of Intuitionism," *The Philosopher*, July 1936.

well defined criterion of constructibility are able to invent an appropriate calculus, our present investigation is still in the more rudimentary stage of knowing simply that "the notation is what we lack" and "the verdict of the mere feeling" is liable not merely to fluctuate but to lead to contradictions.

This part of the discussion should be completed by showing that we are bound to reach the same overlapping of domains and hence the same contradiction, if we were to allow the fringe to be itself bounded by a fringe of higher order and that in turn by another and so on ad infinitum.[22]

It will be sufficient to consider the case of a linear continuum e.g. the set of all geometrical points from a point *a* upon a straight line to a point *b* on the same straight line. If there is a series of fringes each limited by a subsidiary fringe (all composed of points between *a* and *b*) there must be two points *c* and *d*, which may be identical with *a* and *b* respectively, beyond which *no* fringe extends. If we choose *c* and *d* to be as close together as possible,[23] the assertion of Lx will assign x to the interval *a* to *d*, and the assertion of ~Lx to the interval *c* to *b*, these ranges overlapping as in the argument for the finite case. In either case, whether the number of terms in the field of reference is finite or infinite, denial of the existence of a unique boundary between the domains of Lx and ~Lx leads to contradiction. Thus it is impossible to accept Russell's suggestion that the fringe itself is ill-defined.[24] Ill-defined can only mean undefined—there is no place for a *tertium quid* in traditional logic. But an undefined fringe means absence of all specification of boundary between the fields of application of a term and its contradictory—and this is in flagrant contradiction with the facts of the ordinary use of language. *Red* and *yellow* are used as distinct, not identical, symbols in a way which is not seriously affected by the existence of continuous gradations between the two colors.

On the other hand, the awkwardness of assuming a well defined boundary to the fringe is shown clearly in the classical paradox of the heap, sometimes attributed to Zeno (Burnet; *Greek Philosophy*, p. 114, 5). The argument is paraphrased by Adamson (*Development of Greek Philosophy*, p. 38) in this way. "A measure of corn when thrown out makes a sound. Each grain and each smallest part of a grain must

[22] This is Russell's assumption in the paper to which reference has already been made.

[23] The argument would need trivial adjustments if the field of reference, while having an infinite number of terms, did not constitute a continuum.

[24] Op. cit., p. 88.

therefore have made a sound yet no sound is made by a single grain." What is essentially the same argument sometimes appears in modern dress as the paradox of the bald man. Plucking a single hair from a man's head cannot make him bald if he is not so before. But the plucking of all his hair will make him bald and this can be accomplished by the successive pluckings of single hairs.

Both forms of the paradox are associated with the emergence of qualities as a result of successive small alterations in respect of some other (quantitative) characteristic, none of which, except the last, produce *any* change in quality. The repugnance felt towards this type of discontinuity may be merely a prejudice but it seems to be more, and I am unaware of any satisfactory discussion of it. So long as this type of argument is held to apply to a few vague terms the matter is not serious, but if we admit *all* terms are vague its application will invalidate any deductive argument into which it is inserted, and is as awkward for logic as the notorious mathematical antinomies are for mathematics.

The difficulty is serious enough. If we are right in our claim that all material terms are vague, the formal apparatus of logic (and indirectly of mathematics, though this has not been shown) seems to break down. We are unable even to assume that Lx is incompatible with $\sim$Lx, without the assurance that x is not in the fringe, and we are unable to say when the fringe begins and ends. The attempt to assert that x does not belong to L's fringe, say A('Lx') leads us into an infinite regress

$$A\{\text{'}A(\text{'}Lx\text{'})\}, A\{\text{'}A\{\text{'}A(\text{'}Lx\text{'})\}\}, \text{ etc.}$$

and does not evade the difficulty. To say, as Russell does, that "all traditional logic habitually assumes that precise symbols are being employed. It is therefore not applicable to this terrestial life, but only to an imagined celestial existence"[25] is to abandon "traditional logic." If we can "imagine" precise symbols we can construct them—and if "*all* symbols are vague" we cannot even imagine precise ones.[26]

5. IS VAGUENESS SUBJECTIVE?

Subjective is here taken to mean whatever belongs to the processes of cognition, feeling or willing as distinct from whatever belongs to the notion of their object.[27] Suppose an observer O, in the presence of an

[25] Op. cit., p. 88.

[26] Cf. section 9 below when Russell's argument on this point is further discussed.

[27] Cf. article on "Objective," Baldwin's Dictionary of *Phil. and Psych.* II, 192.

environment E, utters a set of words S. We shall say that a feature of S is subjective or objective according as the fact that that feature occured in the situation which consisted of O's enunciating S is evidence for a fact about O or a fact about E respectively. Thus suppose S was uttered in an unusually loud voice; the intensity of the sounds occurring is, in the English language,[28] evidence of O's state of mind (e.g. that he is angry) but not evidence about the ostensible subject matter of his report.[29] Thus the intensity of the sounds in S is a subjective feature. Again if a sufferer from delirium tremens says *There is a pink lizard over there* we are able to deduce only that he is *seeing* a pink lizard (a fact about O), not that there *is* a pink lizard over there (a fact about E). Thus the occurrence of the terms *pink* and *lizard* in his statement are subjective features.

The distinction between subjective and objective features of an utterance is therefore closely connected with the distinction between psychological and physical data and the title of this section can be rephrased as follows: Are the defining phenomena of vagueness, i.e. the variation in the position of the boundary chosen by various observers (or the same observer at various times), facts about human behavior (psychological or sociological data) or facts about the physical world?

The question is best answered by comparison with the corresponding deviation from strict regularities of scientific instruments. For there is no essential difference in this context between the human reporter and the scientific instrument. We can regard the observer as an instrument for making division in a series of objects; the report of his feelings can then be likened to the oscillation of an instrument in a range where direct measurement is difficult.

(Conversely, the scientific instrument always needs a human observer before its readings can be incorporated into the scientific record, so that in a sense the instrument is merely a prolongation and reinforcement of the scientists' sense organs). What is needed is a description of the circumstances in which a variation of the readings supplied by an instrument reporting the character of an object E is ascribed (a) to an "error" of the instrument or (b) to a change in E. The cases (a) and (b) correspond to the variations in the reading being, in the terminology of this section, "subjective" and "objective" respectively.

[28] The qualification is necessary because some languages such as Chinese use differences of pitch as significant linguistic elements.

[29] Since O's state of mind may depend upon his environment, evidence about O's state of mind, may of course, *indirectly* yield evidence also about E.

It is clear that the use of a single instrument I measuring some physical magnitude of an object E and yielding (as all scientific instruments do) various readings on various occasions, is not sufficient for choice between cases (a) and (b) of the last paragraphs. For a character which is present in all measurement cannot serve as a criterion for discriminating between two types of measurement. The method actually used by scientists is of course to keep E and the whole of its environment except I unchanged[30] while using other instruments I′, I″ etc. in order to perform the same readings. If the variations in the readings are systematic i.e. if they conform to a law which is confirmed by all the instruments, the variation is ascribed to a change in E. If for example the length of a body is recorded at intervals of a second as alternately 1 inch and two inches by every instrument, the systematic variation would be interpreted as a periodic alteration in the body's length. In mathematical terminology, the necessary and sufficient condition for variations in the readings of an instrument to be regarded as indication of changes (objective patterns) in the object measured is that the law connecting such variations should be invariant with respect to replacement of the measuring instrument by another of a certain set of instruments. Conversely, if the variations are not connected by an invariant law, they are ascribed to changes in the instrument (subjective factors). This assumption is strengthened if a law can be discovered connecting the variations with the internal structure of the instrument for such a law allows prediction of the nature and amount of the variations irrespective of the nature of E. But in default of such a law the variations may simply be called "random," the essential being that the variations are invariant.

It is for such reasons that in physics gross deviations in measurements of position are ascribed to defects of the measuring instruments, while the joint indeterminacy of position and impulse of Quantum Mechanics, is regarded as objective.

Are the variations in the boundary decisions made by various members of a set of observers analogous to the errors of a scientific instrument or to the variations in the readings of an instrument in accordance with objective variations in the situation measured? We have assumed that the variations are not purely random and that the variant decisions exhibit some statistical regularity. If this is a justified assumption (and without it we are unable to account for the success with which vague symbols are used)—vagueness is clearly an objective feature of

[30] In practice this can never be completely successful.

the series to which the vague symbol is applied.[31] And it will be shown in the next section how the vagueness of the symbolism can be made explicit in a way which ordinary language fails to do[32] and be made in this way to serve as an adequate model of those relations in the field of application from which it arises.

6. DEFINITION OF THE CONSISTENCY—PROFILE

We propose to replace the crude and untenable distinction between fringe and region of certain application by a quantitative differentiation, admitting of degrees, and correlated with the indeterminancy in the divisions made by a group of observers.

The definition involves three fundamental notions: *language* (or *users of a language*), *a situation in which a user of a language is trying to apply a symbol L to an object x*, and *the consistency of application of L to x*. It is impossible to define them in independence of each other, for the first, which is clearly involved in the second and third, is in turn based upon the last. Thus the three notions must be defined in terms of a single process of interpretation, assigning a meaning to any context in which they are used. For the present we shall define a "language" as the vocabulary and syntax abstracted from the laws expressing the uniformity of linguistic habits of a certain group of persons; and that group of persons we call the users of the language.[33] This definition will

[31] It needs, therefore, to be clearly distinguished from such features of symbolism as ambiguity. The latter is constituted by inability to decide between a finite member of alternative meanings having the same phonetic form (homonyms). The fact that ambiguity *can be removed* shows it to be an accidental feature of the symbolism. But any attempt to remove vagueness by a translation is defeated by the over-specification of meaning thus produced. Cf. an attempt to replace *The hall was half full* by *The ratio of the number of persons in the hall to the number of seats was exactly half*. The presence of *one* person too many would falsify the second, but not the first of the statements.

[32] In ordinary language, vagueness is shown explicitly by the use of adverbs of degree or number such as *any*, *many*, *rather*, *almost*, etc. These serve as a set of pseudo-quantifiers, generalisations as it were of the "respectable" quantifiers *all* and *any*, forming a sliding scale which can be attached to any adjective. The method of the next section, which reduces to the conversion of propositional functions into propositional functions of an extra variable by the addition of a numerical parameter is thus the generalisation of a device already present in ordinary discourse.

[33] The "set of conventions" determining the vocabulary and syntax of such a language are the simplified expressions, in the imperative mood, of the empirically discoverable rules of usage. While the existence of such a language presupposes, by definition, *some* uniformity in the linguistic habits of its users, the empirical laws expressing the partial uniformity of such habits are complex, in process of variation, and heterogeneous in

be discussed further in the next section, when it will be shown that the whole procedure is not circular. For the present, however, language will be treated as a relatively unproblematic notion, and we proceed to explain the notion of consistency of application. The method is based on the assumption that while the vagueness of a word involves variations in its application by the users of the language in which it occurs, such variations must themselves be systematic and obey statistical laws if one symbol is to be distinguished from another. It will be necessary to refer to situations in which a user of the language makes a decision whether to apply L or ∼L to an object x. (Such a situation arises, for instance, when an engine driver on a foggy night is trying to decide whether the light in the signal box is really a red or a green light.) Let us call such a situation a *discrimination of x with respect to L*, or a *DxL* for short. (Then a *DxL* will be identical with a *Dx*∼*L*, by definition.)

For some *x*'s, the result of a *DxL* is almost independent of the observer; most users of the language, and the same user on most occasions, decide either that *L* applies or the ∼*L* applies. In either case there is practical unanimity among competent observers as to the correct judgement. For other *x*'s (in the "fringe") there is no such unanimity.

In any number of *DxL* involving the same *x* but not necessarily the same observer, let *m* be the number which issue in a judgment that *L* applies and *n* the number which issue in the judgment that ∼L applies. We define *the consistency of application of L to x* as the limit to which the ration *m*/*n* tends when the number of *DxL* and the number of observers increase indefinitely. (The second number is of course limited to the total number of the users of the language.) Since the consistency of the application, *C*, is clearly a function of both of L and x, it can be written in the form *C*(*L*,*x*).

In a previous section we claimed certain systematic features in the variation of application of a vague but unambiguous symbol. It is now possible to specify these features more exactly. As we pass from left to right along the series *S* of terms *x*, the corresponding values of *C*(*L*,*x*) will have large values at the outset (region of "certain" application of *L*), decrease until values near to one are reached (fringe), and

---

character. It is necessary to distinguish between rules of logic, grammar and good taste. The neglect of certain distinctions and discriminations habitually made by users of the language provides a simplified or "model" language bearing some, but not too much, resemblance to their actual habits. Then the first crude analysis can be corrected by a supplement which considers the facts neglected. Thus the definition proceeds by a series of successive approximations.

decrease again until values near to zero are reached (region of "certain" application of $\sim L$). A list of the exact values of $C(x,L)$ corresponding to each member x of S will be an exact description of L's vagueness. In the figure below, a typical set of consistencies is shown in graphical form.

The numbers along the horizontal axis denote the position of terms in the series S, while the height of a point above a number vertically beneath it represents the consistency of application of the symbol in question for the corresponding term of the series. The points marking the values of the consistencies associated with each member of the series

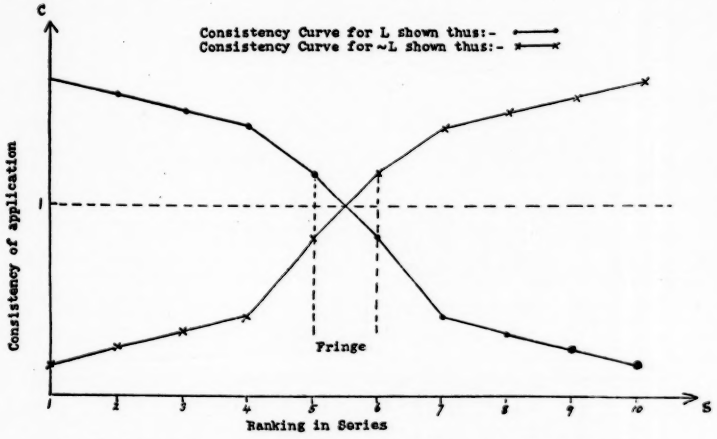


FIG. 1. CONSISTENCY OF APPLICATION OF A TYPICALLY VAGUE SYMBOL

have been joined to form an open polygonal line. It will be convenient to call the curve thus obtained a *consistency profile* for the application of L to the series S. In practice the number of terms in S will usually be very much greater that 10 (e.g. there are said to be something like 700 distinguishable shades of gray) and the consistency curve will approximate to a smooth curve having a continuous gradient.

The exact shape of the consistency curve will, of course, vary according to the symbol considered. It has been assumed that the typical symbol, L, is unambiguous, but an ambiguous symbol will be easily detected by the presence of more than one fringe in its consistency curve. In other words the steady decrease of consistency as we move from left to right

is taken as a definition of unambiguity. Further, the introduction of consistency profile allows us to define the relative vagueness of symbols on the basis of a classification of their corresponding consistency profile. Thus the very precise symbol would have a consistency curve made up of a straight line almost parallel to the horizontal axis, and at a great distance from it, followed by a steep drop to another line almost parallel to the horizontal axis and very close to it, i.e. the curve is marked by the narrowness of the fringe and lack of variation in the symbol's application elsewhere.

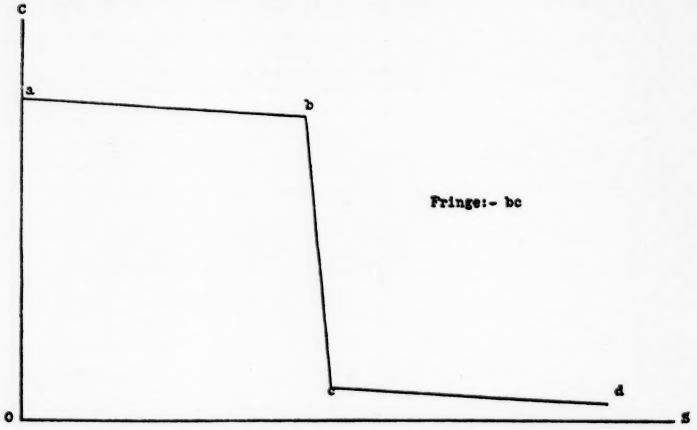


Fig. 2. Consistency Curve of a Very Precise Symbol

The very vague but unambiguous, symbol, on the other hand would have a consistency profile approximating to a straight line of constant negative gradient, i.e. the fringe merges into the whole field and there is continuous variation in the symbol's application (Fig. 3).

Intermediate cases could be classified according to their deviation from the extreme types illustrated in Fig. 2 above and Fig. 3 on facing page.

In order to do this, it might be more convenient to plot not the consistency as above defined, but the deviation of the consistency from the extreme values. Let m be the number of the *more favoured* judgements in a total of n DxL, made as before with respect to the same x. And let Z be the limit approached by $2m/n$ as the number of DxL increases. Then Z lies always between o and 1, is high in the fringe and low in the

regions of certain application, whether of L or $\sim L$. A curve connecting Z with the rank of x would approximate in shape to the frequency curves studied in statistics and the determination of a symbol's degree of vagueness would then reduce to well known statistical problems such as the determination of the area or flatness (kurtosis) of a frequency curve.

It is to be noticed that the existence of a series of relatively less vague symbols does not imply the existence of a symbol of zero vagueness[34] any more than the existence of greater lengths implies the exist-

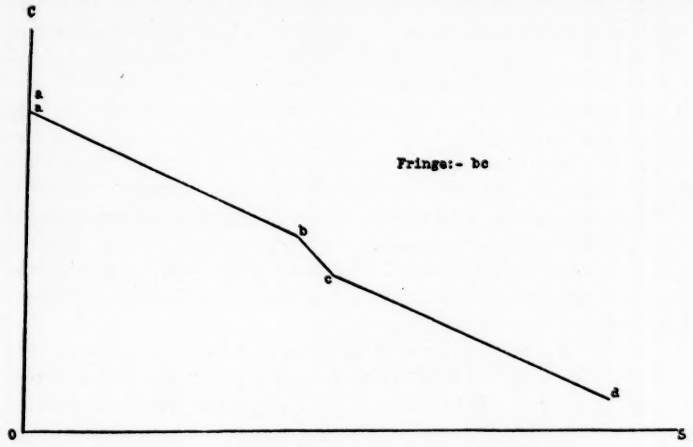


FIG. 3. CONSISTENCY CURVE OF A VERY VAGUE SYMBOL

ence of a greatest or least length.[35] The limits to the application of the term *length* are of exactly the same kind as the limits to the application of *red* or *chair* or any other vague word. It is not possible to set any upper limit to the application of the term *length*, but its applica-

[34] Cp. Russell, *Vagueness*, "we are able to conceive precision; indeed if we could not do so we could not conceive of vagueness which is merely the contrary of precision" (p. 89).

[35] The final term in any case differs from its predecessors in *some* respects. The situation is indeed complicated in physics by the existence of a multiplicity of different methods for measuring length in accordance with the familiar tendency of a science to extend the meaning of a concept by the assimilation of new methods of measurement as they are discovered. But consideration can be restricted to *length measured by a ruler* (the case of length 'in general' produces no difference in principle) and we can imagine this phrase substituted for *length* in the text.

tion becomes less consistent as very large lengths are reached. It is unnecessary in this context to follow the details of the mathematical treatment of vagueness beyond this sketch of a possible procedure.[36]

We have seen that the relations exhibited in the consistency profile can be regarded as equivalent to a numerical function correlating a numerical value of the consistency to each member x in L's field of application.[37] The consistency curves of L and $\sim L$, or the equivalent numerical functions, constitute the complete analysis of the implications of L and $\sim L$ so far as concerns this vagueness. We eliminate the difficulties due to the inadequacy of the dichotomy of Lx and $\sim Lx$ by providing a more adequate symbolism in which explicit account is taken of those quantitative relations in the field of reference of which the difficulties in interpretation of the dichotomy are a sign.

If the analysis of L, (i.e. the specific consistency profile) be denoted as L', an alternative mode of formulation would be to regard the consistency distribution as indication of the *degree* to which L', the more explicit symbol is applicable to the corresponding terms of the series S. We then regard L in its analysed form as the incomplete expression of a propositional function having *two* arguments, reading $L'(x, C)$ as *$L'$is present in x with degree C.* In this form of expression attention is drawn to the objective relations between L' and S which determine the consistency distribution.

To remove a possible source of misunderstanding it may be as well to add that the analysis of Lx in the manner suggested does not involve the claim that a person asserting Lx in a DxL should know the analysis, i.e. the corresponding distribution of consistencies of application, either at that or at any subsequent time. Any assumption that ability to use a symbol correctly involves extensive statistical knowledge of the behavior of other users would involve a vicious circle. But we can very well use a symbol correctly, i.e. in statistical conformity with the behavior of a certain group of users, without knowing in detail to what we are committed by the linguistic habits of the group.

[36] Cf. Appendix II for further details.

[37] On a frequency theory of probability the assertion of the value of $C(L, x)$ for a given argument x could be interpreted as an assertion concerning the probability of L's application to x. This would involve interpreting every statement of the form Lx as statements of probability lacking a numerical parameter. Such a theory bears a formal resemblance to the theories of say Keynes or Reichenbach (*Wahrscheinlickheitslehre*). But the argument in the text is independent of any particular interpretation of probability.

7. DEFINITION OF THE USERS OF A LANGUAGE

The sense in which language is used in the previous section is clearly a technical one, which needs further discussion.

We need a sense which is narrower than the ordinary sense in which French, Italian or German are called languages. For "In a country like France, Italy or Germany . . . every village or, at most, every group of two or three villages, has its own dialect. . . . The difference from place to place is small but, as one travels in any one direction, the differences accumulate, until speakers, say from opposite ends of the country, cannot understand each other, although there is no sharp line of linguistic demarcation between the places in which they live."[38] Thus "correct" German or "correct" Dutch are better regarded as specially important dialects abstracted from languages which shade into each other by a continuous series of intermediate local variations.[39] We need a sense however which is slightly narrower even than that of the "official" dialect. For if it is a question of obtaining a consistency profile for the name of a color, we shall want to exclude certain persons who cannot speak the official dialect. The observations of the color-blind or those who claim to perceive distinctions in shade invisible to all other persons are valueless in constructing the profile. Thus, without attempting to achieve the empty ideal of strict uniformity we shall find it necessary to exclude from the group of users of the language persons having unusual powers of discrimination. The element of arbitrary convention in the use of the term language in our technical sense is unavoidable but characteristic of all attempts at definition. If however, in view of the enormous variation on geographical, social, technical, and even sexual grounds to be found in lingustic behavior we reject the assumption of *strict* uniformity as too crude an approximation to be useful, it will be necessary to specify how the amount of *permissible* variation is determined. We shall do this by using the process of deriving the consistency profile of a vague symbol simultaneously as a definition of the privileged users of the symbol.

We can begin by considering such a large group of persons that the problem reduces to that of identifying some selection from the group as *the* set of users of the symbol *L*. It has been assumed that *no* set of persons thus selected (not even a set consisting of a single person)

[38] L. Bloomfield, *Language*, 1935, p. 51.
[39] Bloomfield, *cf. cit.*, p. 44.

will show absolute uniformity in applying L. For each set,[40] however, we can apply the procedure of the preceding section, making the number of DxL for each x increase indefinitely, for each x in turn, but keeping the group of observers constant. In this way *some* of the subgroups will provide their own characteristic consistency profiles for the symbol L.

Now the various sub-groups can be classified and amalgamated on the basis of the mutual deviations of their respective consistency profiles. In the simplest case in which the sub-groups separate into a number of non-overlapping classes, each having exactly the same consistency profile, we say that there are as many different usages of L as there are distinct forms of consistency profiles produced by this statistical analysis, and define *the* sub-group of users of L in any one of its meanings as the *largest* sub-group having the corresponding consistency profile.

In practice, however, the situation is likely to be complicated by the existence of a great many consistency profiles with gradations between extreme types. In this case the notion of privileged users whose behavior determines the vagueness of L' will itself be a relatively vague notion. The decision whether to extend any provisionally selected sub-group by the inclusion of new members will depend upon the modifications in the shape of the consistency profiles which such admissions entail. When such modifications are slight the group will be extended, but not otherwise. Newcomers whose admission would entail radical modifications in the shape of the profile will be said to use the terms L and $\sim$L in a different way from that of the group already established. Thus the process of selecting a group of users and of discovering the consistency profile of a symbol are complementary and interact upon each other.

The whole analysis may be compared with the process by which species are defined in biology. There, too, the group of animals constituting a species is defined not by exact correspondence in habits or characters, but by a statistical distribution of variation in habits around a mean position.[41] When a grouping of properties round a mean posi-

[40] Actually to do this would seem to involve examination of the limiting behavior of *each* sub-group. The practical difficulties this would involve and the subsequent modifications required in practice in determining the uses of a language in any specific case need not be considered in a discussion of the principles.

[41] Thus if we adopt a recent definition of species in genetic terms as a "group of individuals fully fertile inter se, but barred from interbreeding with other similar groups by

tion is discovered, it is regarded on the one hand as the *definition* of the species, and on the other as a *property* of the species once it is defined. So too statistical regularity in deviations of linguistic habits is used both to isolate a privileged group and simultaneously to define those habits by the invention of an appropriate symbolism.

APPENDIX I. AN EXPERIMENT IN VAGUENESS

It was hoped that the following experiment might illustrate the formation of a consistency profile and the manner in which consistency of application can be an index of objective relations in the field of reference. Each subject was asked to make a single division, at what seemed "the most natural place," in the series of rectangles shown below, and, having made a unique choice, to analyse the reasons for his decisions. (The instructions used are reproduced at the end of this appendix, so that the results obtained can be checked by others).

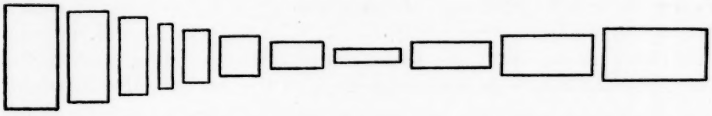


FIG. 4. SERIES OF RECTANGLES USED IN EXPERIMENT ON VAGUENESS

Careful inspection of the series will show at least three[42] criteria for dividing the series which are covered by the deliberately vague word "natural" used in the instructions for the experiment. The rectangles diminish in height by equal steps from left to right until the eighth from the left is reached, and then increase in height again by the same amounts until the end of the row is reached; exactly the same is true of the *breadths* of the rectangles considered in the opposite direction from right to left. In addition to this the heights and breadths are so

its physiological properties (producing either incompatibility of parents or sterility of the hybrids or both)" (T. Dobzhansky, 'Critique of the Species Concept in Biology,' *Phil. of Sci.*, II, 353) we are compelled to admit the qualification that "neither the mechanisms providing incompatibility, nor those producing sterility, function on an all-or-none principle. For instance, sexual isolation may be incomplete, and individuals belonging to different groups may sometimes, though seldom, copulate. Similarly, some hybrids are only semi-sterile or sterile in one sex only" (Ibid.). Nor is the situation fundamentally altered if a genetic definition of this sort is replaced by a taxonomic definition in terms of the possession by the members of the species of certain common characteristics.

[42] In fact, of course, there are an indefinite number of criteria which might be applied; those described are merely the most 'natural' i.e. those which most people tend to use.

correlated that each rectangle is geometrically congruent to another rectangle distant the same number of places from the nearer end of the series, and is obtained from it by a revolution of 180° about an axis perpendicular to the paper. Thus the first and eleventh rectangles, the second and tenth, third and ninth rectangles etc., have the same shape and size. If the criterion of diminishing heights be called *H* for convenience of reference, the criterion of increasing breadth *B* and the criterion of symmetry (which is a kind of combination of *H* and *B*) *S*, it will be seen that application of *H* would result in a decision at a point between *7/8* and *8/9* inclusive (see instructions at end of this appendix for explanation of the notation), application of *B* at a point between *3/4* and *4/5* inclusive, application of S at the Point 6. Thus conflicting criteria produce overlapping fields of application, as in the case of the color spectrum. When the experiment was performed on 83 persons it was found that criterion *B* was seldom employed, the most usual reaction being an application of *H*, *S* or a compromise between the two.[43] Although no particular care was taken to insure homogeneity of the group of subjects, who were in fact of all ages and types, drawn from the writer's acquaintances and students, the corresponding consistency profile, even with the small number on whom the experiment was tried, has quite a distinctive shape of the kind associated with a simply ambiguous symbol. It is to be expected that the characteristic concentration of the number of divisions at or near *6* and *7/8* would be preserved as the number of subjects increased.

Table of experimental results

PLACE AT WHICH DIVISION WAS MADE	NO. OF PERSONS MAKING THE DIVISION AT THAT PLACE
1 to 3/4	None
4	1
4/5	2
5	None
5/6	4
6	36
6/7	6
7	4
7/8	16
8	14
8/9 to 11	None
	Total: 83

[43] Any tendency to make a division in the middle of a series could have been avoided by prolonging both ends of the series a considerable distance or by using a series pasted round a cylinder.

(TYPEWRITTEN INSTRUCTIONS ISSUED WITH THE SERIES OF RECTANGLES)

You are supplied with a series of rectangles arranged in a horizontal line. Do not turn the sheet round; keep them in the horizontal position. You are asked to divide the set of rectangles by a *single* vertical line, at what seems the most NATURAL place. The division may be either between two rectangles or through the middle of one of them. E.g. if all rectangles to the left were red and all the rest were black, the 'natural' place to make the division would be between the red and the black rectangles. *Do not draw any lines*, but show your decision on a separate sheet of paper in the following way:

A division between the third and fourth rectangles from the LEFT is shown by writing 3/4, and so on.

A division through the middle of the tenth rectangle from the LEFT is shown by writing 10, and so on.

You can take as much time as you like, but must make one and only one division.

(The following was issued after the first part of the instructions had been performed):

If you can, try to explain in writing *why* you made the division in the place you did, and add any comments you think interesting (e.g. whether you hesitated between several places).

(Actual dimensions of rectangles: 4 cm. x 2 cm., 3.5 cm. x 1.5 cm., etc. (unit of increase in linear dimension 0.5 cm.). Space between rectangles: 0.5 cm.)

APPENDIX II. EXTENSION OF THE ANALYSIS TO THE LOGICAL RELATIONS BETWEEN VAGUE SYMBOLS

One of the main problems with which this essay has been concerned is the applicability of logical principles when vague symbols are involved. The notion of an ideal universe in which the laws of logic and mathematics have unconfined validity having been rejected, it remains to show how the undoubted usefulness of the formal sciences in a field of vague symbols can be explained by an extension of the method already sketched in the earlier sections.

From the formalist standpoint, the analysis of vagueness in terms of consistency functions can be regarded simply as the introduction of more complex symbolism, replacing the propositional function Lx of a single variable, by a function of two variables, $L(x,C)$, (read: "L applies to x with consistency C"). The relations between symbols in a calculus whose symbols are assumed to be "absolutely precise" will then appear as a limiting case of the relations between symbols having an extra argument, "C", and obtained from the general case by allowing C to tend either to zero or to infinity in every formula in which it occurs,

i.e. in effect simply by suppressing that argument.[44] Thus the validity and usefulness of the relations applicable to the limiting case (logical relations between "absolutely precise" symbols) will depend upon the degree to which they can be represented as a standard to which the more general case approximates. In particular, the pure logical relations between the incomplete symbols attained by suppressing parts of vague symbols would appear as limiting cases of relations between vague symbols.

The generalization of the usual notions of material implication or negation of propositional functions of a single variable will be relations connecting the corresponding values of the consistency arguments in two propositional functions. It follows from the definition of the consistency function that if $L(x,C)$ and $\sim L(x,C')$ for the same x, the products of the two consistencies, C and C', is unity. Thus the principle of excluded middle is replaced by the operation which permits the transformation of $L(x,C)$ into $L\left(x,\frac{1}{C}\right)$.[45]

The consistency profiles of vague symbols can in fact be regarded as a generalization of the circles in the Euler diagrams traditionally used by logicians to represent the relations of inclusion and exclusion between classes.[46] Just as the operation of negating a propositional function corresponds in the usual spatial analogy to the movement from the interior of a circle to its exterior, so also in the spatial illustrations of this paper, the corresponding transition is from L's consistency profile to the reciprocal curve for $\sim L$(cf. Fig. 1).

[44] The limiting process and the suppression of the argument are assumed to produce equivalent effects.

[45] If this principle of transformation is itself formalised (corresponding to the use of the law of excluded middle as a premiss as well as a logical principle) it will be necessary to introduce a further consistency variable. The generalisation of the assertion (X) (Px v $\sim$ Px) will then be

$$(x, c)\ \{P(x, c)\ v \sim P(x, f(c))\}$$

Where f is some specified function of c (nearly equal to c when C is near to 1, very small when c is large and very large when c is small. The exact form of $f(c)$ would depend on the exact form of the consistency curves in the special case.)

[46] Since such diagrams habitually assume a two-dimensional field of application, the corresponding diagram of consistencies of application should strictly be three-dimensional and consist of a (polyhedral) surface obtained by joining the top of adjacent ordinates erected not upon an axis (OS in Fig. 2) but upon a plane of reference. The argument of the section can, however, be sufficiently illustrated by supposing that the field of application is a one-dimensional series as in Figs. 1–3 above.

This in turn suggests a generalization for the relation of implication between propositional functions, or the equivalent relation of inclusion between their extensions. We take as the *type* of the case when L's field of application includes M's field the case in which M's consistency profile lies wholly *underneath* L's consistency profile, i.e. for every x, M's consistency of application is less than L's consistency of application to that x. It is necessary to say that this definition specifies only the type or standard case of the relationship between two functions, because, in accordance with the general standpoint of this paper, deviations from the type must be allowed. If the consistency of M is greater than the consistency of L in *very few* of the x's, we shall still say $Mx \supset Lx$ to *some* extent. We can imagine a number $i(L,M)$ which measures the degree to which the relation between the consistency profiles of L and M deviates from the standard case of inclusion.[47] Then i, which might be called an approximation index for the relation of inclusion is a function of the consistency distributions of L and M.

Thus the statement that $Mx \supset Lx$ will be generalized into some such form as

$$\supset\{i(L,M),\ c\}$$

i.e. a propositional function of two variables, viz. the approximation index for inclusion, and the usual consistency variable. Hence the syllogistic law which permits of transition from the two formulae

$$\supset(L,M) \text{ and } \supset(M,N)$$

to the formula

$$\supset(L,N)$$

is generalized into some rule connecting the different approximation indices in

$$\supset\{i(L,M), c\},\ \supset\{i'(M,N), c\}$$

and

$$\supset\{i''(L,N), c\}$$

Thus if the approximation index were suitably defined it might follow that i'' must be $< i + i'$. The rule for passing from $\supset(L,M)$ and

[47] i(L, M) might be, for example, the ratio of the number of x's when M's consistency is greater than L's to the number when M's consistency is less than L's. The exact definition which is chosen is unimportant.

$\supset(M,N)$ to $\supset(L,N)$ would then become: Whenever $\supset\{i(L,M), c\}$ and $\supset\{i'(M,N), c\}$ are asserted for the same value of c and specific (but not necessarily identical) values of i and i′, then $\supset\{i''(L,N), c\}$ can be asserted, with the same value of c, the value of i″ certainly being less than the sum of i and i′. In the special case where $i = i' = o$ (strict inclusion) we shall have $i'' = o$, and the ordinary relation will hold.

We can characterize the preceding interpretation very roughly in this way: the ordinary rules for the logical transformation of sets of statements (e.g. the syllogistic rules) produce conclusions whose degree

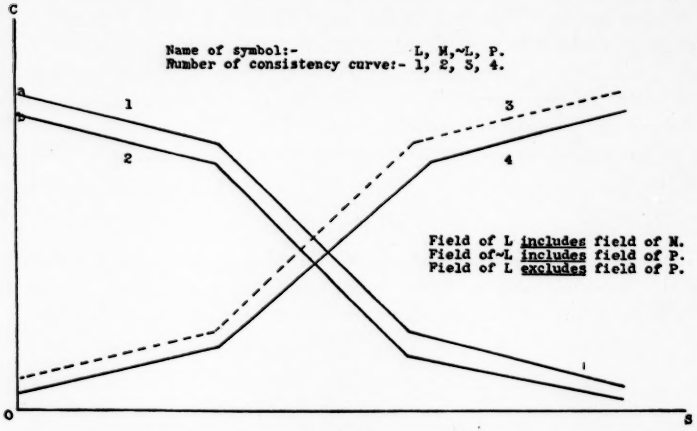


Fig. 5. Logical Relations between Vague Symbols Spatially Illustrated

of vagueness is of the same order[48] as those of the premisses. In proportion as the relations between the consistency curves approximate to the definitions of inclusion and negation given above, the resulting laws of logical transformation approximate to those of the traditional forms.

By means of the notions of inclusion and negation it is easy to define the relation of *exclusion* between consistency functions. We say that *L*'s field *excludes P*'s if ~*L*'s field includes *P*'s. The standard cases of the relationships arising are shown in the figure below. It is easily verified that the new definitions of inclusion, exclusion and negation

[48] This means roughly speaking that it tends to zero when the degree of vagueness of all the premises tend to zero.

preserve the usual formal properties of these relationships. Thus both inclusion and exclusion remain transitive: also if L's field includes M's, $\sim M$'s field includes $\sim L$'s etc. (See Fig. 5 on preceding page.)

Thus, if the logical forms are interpreted as standard cases to which the relations between consistency functions may approximate, the formal properties on which the theory of formal logic are based remain valid in the approximatory sense discussed.

For while the vagueness not only in the terms of the premisses but in their relations prevents us from asserting the conclusion of an argument in applied logic or applied mathematics without a qualification as to the degree of consistency (whose amount depends on the precision of the terms and logical relations) the *form* of the transformation is independent of the actual consistencies provided we are satisfied with a final precision which increases indefinitely when the precision of the premisses increases.

University of London
Institute of Education.

A Critique of Operationalism in Physics

BY

R. B. LINDSAY

IT IS the aim of this paper to examine certain aspects of a point of view which has attracted much attention in physical methodology. This is the standpoint known as *operationalism*.[1] We wish to discuss its significance in the construction and interpretation of physical theories.

OPERATIONALISM

The essential meaning of operationalism in physics is that physical concepts should be defined in terms of actual physical operations. On this view there is no meaning to a concept unless it represents an operation which can be performed in the laboratory. Thus the term "pressure of a gas" signifies nothing until an operation is described which constitutes the measurement of pressure. When one thinks of pressure one must think of some actual apparatus, such as, for example, some glass and rubber tubing and some mercury and certain operations performed using these until finally one gets a pointer reading which one calls the pressure of the gas. The same stand is taken with respect to length, time, velocity, acceleration, temperature, etc.

It should perhaps be stated in all fairness at the outset that the above is an extreme statement of the operationalist's point of view. He

[1] The principal exponent of operationalism in physics is P. W. Bridgman, whose books are well known: "The Logic of Modern Physics" (Macmillan, N. Y., 1927) and "The Nature of Physical Theory" (Princeton University Press, 1936). In these will be found references to numerous articles where his views are further expounded. The present article deals principally with Bridgman's writings.

usually does not deny that it is possible to define physical concepts by other means than by physical operations. But he insists it is only by means of operationalism that we can hope to avoid inconsistencies and contradictions. The attitude expressed here is a very definite one, namely, that no contradictions can ever arise in actual physical situations. Hence we shall always be safe in subjecting all concepts used to the test of such situations. Let us examine this thesis for a moment.

There is no question that experimental physicists feel keenly the *definiteness* associated with experimental observations. Indeed this feeling is shared by all physicists. Nevertheless the very fact of the existence of controversies over some of the important experimental discoveries of our own and past times should make it clear to us that this definiteness is not won save at the cost of a great deal of thinking or purely mental planning into which, in fact, a great deal of theorizing goes. Consider, for example, the controversy that accompanied the entrance into the realm of accepted physical phenomena of the Compton effect. Or take as another illustration the attempt to use the magneto-optic effect for chemical analysis and particularly the detection of isotopes. This is still in a decidedly controversial stage. Many experimental results now generally accepted prove on further closer investigation to be very questionable. A few years ago nearly every one thought that the value of the charge on the electron was settled for all time, though to be sure some people thought they had detected the existence of a sub-electron, or at any rate charges considerably smaller than 4.77×10^{-10} e.s.u. In the past few years, however, the whole question of the precise value of the charge has again become an open one. Where, we may ask, in all this is the definiteness, objectivity and freedom from inconsistency which are so loudly boasted for purely experimental operations? Of course, it will probably be objected that there is no real contradiction involved in the experiments, but merely in their interpretation. The answer to this is plain: what good are experiments and laboratory operations generally except for the meaning we give them? We do not go through such operations aimlessly, purely for the pleasure of observing things happening. We perform experiments ultimately with certain ideas in mind with the hope of establishing the existence of routines of experience. But the experiment is worthless without the description of it in terms of what we think we are doing.

It seems clear then that the argument that all physical concepts should be based ultimately on laboratory operations, because it is only

in the case of the latter that we really know what we are doing, is not convincing. On the other side weighty objection can be raised to a thoroughgoing adherence to the operational method. This is none other than the conviction that such adherence would necessarily imply the abandonment of the method of theoretical physics, which in spite of difficulties has on the whole proved itself enormously successful in the description of physical phenomena. For logically the operational method rigorously carried out implies that each concept is tied to a definite operation. Thus the pressure of a gas as measured by the ordinary U-tube manometer must be a different concept from the pressure as measured by an ionization gauge, since the operations are radically different. But such an isolation of concepts would defeat the very aim of physical science, which is to provide a simple and economical description in terms of a minimum number of concepts of the portion of human experience which we agree to call physical. Actually the concept of pressure is derived from the theory of mechanics and it is on purely theoretical grounds that we decide that various methods may be used to assign numerical values to the symbol representing it. It has been and should be the physicist's steady purpose to keep concepts from a too close association with a particular operation. The concept of temperature provides another illustration. When first introduced it simply meant a number obtained by the use of a particular thermometer. But under apparently identical conditions different thermometers give different results so that the concept must be arbitrarily associated with a particular operation. Much effort has been expended on this problem, culminating in the absolute thermodynamic scale of temperature which is independent of the particular thermometric substance used. Even more recent is the attempt to define the concept statistically. Now it is hard to believe that these purely theoretical constructions have not deepened our understanding of thermal phenomena. Nevertheless strict adherence to the operational point of view would place them outside the pale.

Now it must not be supposed that in criticizing thoroughgoing operationalism, we are recommending the "emancipation" of theoretical physics from the test of experiment. This is far from our intention. The ultimate goal of every physical theory is to describe physical phenomena; hence the deductions from the successful theory must be in the form of laws verifiable in the laboratory. What we are insisting on is the right, which has indeed justified itself repeatedly in the past, of using in the construction of theories, concepts which are not defined

directly in terms of laboratory operations. Such a one, for example, is the potential energy function of classical mechanics, and such again is the state function of quantum mechanics. Such are the electric and magnetic fields in the electron theory of matter, concepts which are literally defined in terms of the equations their representative symbols are assumed to satisfy.

THE USE OF MATHEMATICS IN PHYSICS

How is mathematics used in physics? According to the operationalist when a theoretical physicist builds a theory of a particular physical phenomenon he ultimately presents a set of equations which are supposed to contain the results of his cogitations; but he usually forgets to supply the "text" without which the equations are meaningless. Just what is this "text"? It is a set of verbal statements which give the operational meaning of every symbol in the equation, i.e., directions how to assign a number to the symbol by measurement. The text must also tell *what* equations to use and *when* to use them. Now all this is, of course, true, but it seems apt to give a misleading impression of the nature of physical theory as actually used.

A physical theory is but an attempt to *describe* a certain realm of phenomena. Its procedure is to start with certain intuitive and logically indefinable concepts and from these to build more elaborate concepts by purely postulational methods. For example, to use mechanics as an illustration, out of the primitive, indefinable space and time concepts are constructed the more sophisticated abstract concepts of velocity and acceleration. It is not denied, to be sure, that ideas gained from experience and in particular laboratory operations enter into this process. Inevitably, however, there is and always will be an element of arbitrary choice in the construction of concepts: we build those which we believe are going to be useful to us in physical description, even if they do not always correspond closely to raw experience. Intuition and free imagination play more of a rôle here than is sometimes supposed. Having constructed the concepts and assigned symbols to them, we continue the process of theory building by assuming certain mathematical relations among the symbols. Usually these assumed relations are in the form of differential equations, since there is an urge to apply the postulates to *elementary* parts of experience: this is the content of what has been called the principle of elementary abstraction.[2] From these differential equations by the process of

[2] Reference may here be made to "Foundations of Physics" by R. B. Lindsay and H. Margenau (New York, Wiley, 1936).

integration are obtained those relations among the symbols representing actually measurable quantities which we call the *laws* for the set of phenomena in question.

In any discussion of the place of mathematics in the building of physical theories, it appears essential to distinguish carefully between the assumed principles and the derived laws. Since the latter are to be the immediate description of phenomena they must be in terms of quantities corresponding to laboratory operations. This, however, is not at all necessarily true of the symbols entering into the hypothetical principles. We have already mentioned the fundamental equations of of the electron theory of matter and the vectors **E** and **H** which they contain. The most reasonable interpretation of **E** and **H** is that the equations serve to define them; indeed the usual operational definitions of the field quantities in terms of force on unit charge or pole break down entirely when one is discussing the field over a single electron. Even in macroscopic electromagnetic phenomena the field quantities as forces on charges or poles do not enter into the final laboratory equation: actually we measure potiental gradients or forces or torques between current carrying conductors or between conductors and streams of charged particles. In these final equations **E** and **H** have disappeared. Yet we still believe that they are extremely useful concepts and would not think of trying to construct the theory of electromagnetism without them.

Even in the laboratory equations which constitute physical "laws," not all the quantities entering are directly measurable. Thus in the familiar law of motion for a freely falling particle, viz. $s = \frac{1}{2} gt^2$, the quantity g gets its meaning not by any direct measurement, but solely because of its appearance in the formula. We must use the formula to calculate it—another indication of the importance of theory in every experimental operation.

Instead of considering the equations representing physical laws as meaningless without the "text" (in the operational sense) it seems much more just to look upon the equations as the best symbolic description we have *of* the text, where by the text we *here* mean the complete verbal description of the operations performed in the laboratory in the phenomenal range in question. No one doubts that we can express physical laws in terms of the language of everyday speech. However, for reasons of economy and power, we prefer to describe what we do in terms of the more abstract symbolic language of mathematics.

The recognition of this point of view would prevent one, for example,

from making or assigning particular significance to the statement: "Mass can have no explanation in mechanics." It is not the function of classical mechanics to give an *explanation* of mass. Indeed, in the usual philosophical sense of the word, no physical theory ever ultimately *explains* any physical phenomenon. It merely provides a description of it in terms of something else. The theory of mechanics gives a *definition* of mass, which is all that is necessary for its use. Of course, this does not prevent us from building a theory of the constitution of matter in which the mass of a material particle is postulationally connected with the properties of electric charges. If we like we may call this an "explanation" of mass. Actually it is still only a description of a certain set of phenomena in terms of another set.

During the previous centuries it was considered most desirable in framing the hypotheses of physical theories to utilize as far as possible the suggestions of immediate experience. In this connection one at once thinks of the dynamical theories of various phenomena employing mechanical models. The reason for this is not surprising: the theory of mechanics is so old and has been so successful that there has grown up an instinctive tendency to think in mechanical terms. Our familiarity with this subject has reached the point where its ideas appear more reasonable and so more "true" than other less familiar physical concepts. Hence arises the further tendency to distinguish between mechanical theories on the one hand and those theories in which the postulates are non-mechanical. There is no logical basis for this attitude. In taking it we forget that the construction of the theory of mechanics is logically of precisely the same character as the construction of any other physical theory. The meaning of physical "explanation" is the same in both cases and the one type of theory is a no more genuine "explanation" than the other.

In his analysis of the use of mathematics in the construction of physical theories the operationalist is apt to be disturbed by the fact that the mathematical equations used in physics contain no "governors" to limit the validity of their application to the actual range observed. Thus the equations of mechanics are quite willing to answer the question how far a certain particle has gone in 10^{-100} seconds or 10^{100} seconds, although we do not attach significance to the result. The operationalist would see therein another illustration of the necessity of the "text" to tell us over what range of variables the equation is really descriptive of the phenomenon in question. However, closer inspection reveals that the difficulty raised is not confined to mathematical description.

We have already emphasized that all description is but an attempt to convey meaning about one set of things in terms of another set of things. Consequently, no matter how hard we try to make our physical description precise, it must clearly understate or overstate. When we describe an electron, for example, we can only tell how it is like or unlike something with which we feel ourselves to be more familiar, such as a marble or a bullet. From the standpoint of the way in which physical theory is actually used the equations of the electron theory provide a much better *description* of an electron than any purely verbal statement.

MODELS

The use of models in constructing physical theories is a very old scheme. It has appealed particularly to those who feel that as soon as a thing can be described in terms of a mechanism, it is understandable. Atomic theory was for a long time a favorite play ground for modelists. Even the Bohr theory of atomic structure is commonly referred to as the Bohr or Bohr-Rutherford atomic model. But this brought in a rather queer circumstance: while the earlier models, even when complicated, were based definitely on classical mechanics, the Bohr "model" departed completely from this ideal by introducing non-mechanical elements, viz., the stationary state concept and the quantum conditions. Nowadays with the advent of quantum mechanics the word "model" has disappeared from the literature of atomic theory, except perhaps in elementary teaching. It is quite possible, indeed, for an operationalist to insist that the quantum mechanical theory of the atom is a model, just as much as the Bohr theory; only it is a "mathematical" not a "physical" model. This is essentially the viewpoint of Bridgman and in his latest book[1] he discusses at some length theories employing free mathematical construction under the heading "Mathematical Models." The attempt is made here to distinguish between so-called "true" and merely "possible" theories. Presumably a "true" theory is one in which the fundamental assumptions are directly verifiable by operations, while a "possible" theory is one where this procedure is impossible. Thus classical mechanics would appear as an example of the former class and quantum mechanics of the latter. Closer inspection shows, however, that this attempted distinction violates the interpretation now generally held of the fundamental nature of a physical theory: that its fundamental principles are always postulates which are unverifiable by direct observation and receive their verifica-

tion only through the experimental test of the laws derived from them. As we have already insisted, from this point of view there can be no essential distinction drawn between classical mechanics and quantum mechanics. As far as the postulates go, neither theory can be ranked as "truer" than the other. The fact that the postulates of classical mechanics sound more familiar to us and seem to have an instinctive appeal can have logically nothing to do with their greater "truthfulness." It is a moot question just where the word "truth" belongs in a physical theory. Most physicists are probably ready to admit that there is such a thing as a "true" description of an actual operation as distinguished from an erroneous one, but the only test of the "truth" of a theory would appear to be the agreement between the laws derived from it and those experimentally observed. This leaves quite open the possibility of a number of equally "true" but different theories of a certain range of physical phenomena. This situation has been well known in physics.

It is of course possible to construct a theory with postulates so "ad hoc" that it describes a very limited range of experience. Such a theory, however, would hardly be considered satisfactory unless it could meet the usual criteria imposed as a test of the success of a theory, viz., the prediction of new experimental results and their subsequent verification. A theory that makes no prediction which is within reach of experimental verification is no physical theory. Probably the thing which has made many physicists most suspicious of the relativity theory of gravitation is the relative paucity of experimental tests, in addition to the fact that its predictions differ so little from those of the classical theory that the experimental results are all second order effects and extremely difficult to pin down with accuracy. Nevertheless even the relativity theory is following the course of evolution of all physical theories: search for the verification of new predictions is constantly being made in the domain of cosmology, where the differences between its predictions and those of classical theory may be expected to be most marked.

It would indeed be gratifying to find a *unique* theory for a given domain of physical experience. From the fundamental philosophical point of view, this may be considered the *real* problem of physics. We can feel quite certain, however, that it never will be solved except perhaps for domains so artificially restricted that we are not interested in them. It must not be forgotten that preferences of taste and fashion

have a great part to play in the development of physical theories. This has been ably set forth by Schrödinger[3] in a discussion of the influence of environment on trends in physics. What appears to us to-day to be a well thought out program for the systematic development of physical theory may to the scientific historian of the future appear merely as the expression of narrow prejudice wasting time in the vain pursuit of a futile idea. The only protection against this is to maintain as broad a point of view as possible, and in particular always to look for the essential equivalence of what appear to be different points of view.

It appears to Bridgman[1] that physics is about to be deluged with a wave of theories employing abstract "mathematical" models, or as he terms them, "possible" theories. For this he advances the following very interesting reason. The new indeterminacy principle of quantum mechanics sets a fundamental limitation to the accuracy of physical measurement. Consequently it is possible to establish a number of physical theories which when applied to a particular phenomenon all lead to numerical results lying within the same nebulous region of indeterminacy. Therefore we shall never be able adequately to test these theories so as to prefer one to the others and we shall ultimately be overwhelmed with embarrassment as to what is and what is not physics. This is, to be sure, theoretically possible but actually highly improbable. In any case, if such a situation were actually to arise, we should meet it by doing what has often been done before: seek for new experimental evidence against which the competing theories can be tested. It will very rarely happen that in every case the difference between them will lie in the indeterminacy zone. No matter how often it does this is no *proof* that we shall not ultimately find an *experimentum crucis*.

There is indeed another aspect of this matter which indicates how unsafe it is to dogmatize. It must not be forgotten that the indeterminacy principle which is here the "devil of the piece" is itself a deduction from a theory, namely, the theory of quantum mechanics. But it will be said that this deduction can be tested experimentally. This is granted, but if there really exists in all measurements the fundamental indeterminacy, how is the principle to be verified so exactly that we can definitely rule out possible alternative forms of quantum mechanics which predict slightly different amounts of indeterminacy? It must be remembered that quantum mechanics does not merely say that an indeterminacy must exist. One can calculate in any particular

[3] Cf. his "Science and the Human Temperament" (Norton, N. Y., 1935).

problem the precise magnitude of the indeterminacy. From a logical point of view there thus appears a certain difficulty in the use of the indeterminacy principle for the purpose intended. This observation is not intended to suggest that the principle has no value: certainly it can actually be used in a very practical way to make estimates, for example, of the average half-widths of spectral lines. Moreover, it may prove to have even greater theoretical power: it is possible that, if stated precisely, it may be used as an actual postulate for the construction of a form of quantum mechanics, as Lindemann[4] has suggested, rather than appear as a deduction from the theory. So far as the author is aware, no one has worked out this point of view in detail.

It is not surprising that an operationalist in physics should flirt with the idea of "emergence," which has played such a significant rôle in biological thought. The difficulty of accounting for the properties of a living organism in terms of its component elements leads naturally to the assumption that the organism possesses properties (so-called "emergent" ones) which do not follow from an integration of those of the elementary constituents. Applied to physics this might mean that the properties of matter in bulk should not be simply the integration of those of the constituent atoms and molecules, or further that the properties of an atom should not be derivable from the properties of the component nuclei and electrons, etc. If this *is* the meaning of emergence in physics, it does not make sense, since the whole purpose of *inventing* atoms, molecules, nuclei and electrons is to enable us to describe the large scale properties of matter. Hence what object would be gained by going to the trouble of conceiving these constituent elements and then not expecting to obtain from them the properties they were constructed to describe, postulating rather something wholly independent of them?

Now as a matter of fact it must be admitted that physics does indeed use descriptions of the behavior of bodies which do not depend on the constitution of matter. For example, the method of thermodynamics is of this definitely macroscopic character. It is based on certain fundamental principles, assumed to be universally true, and employs parameters defined from matter in bulk (e.g., pressure, volume, temperature, energy, entropy, etc.). To this extent physics already makes use of the idea of "emergence," though not in the precise sense of the biologist, for the latter can see and examine in detail the behavior of the elementary cells of the organism. However, it is of great interest to

[4] Cf. his "Physical Significance of the Quantum Theory" (Oxford, 1933).

observe that the physicist has never been wholly contented with any purely macroscopic theory. He has always felt the urge to provide a *structure* for matter and then to interpret the behavior of matter in terms of this structure. It appears that the only reasonable argument for the use of emergence in physics is to insist that this latter method, which we may call the *microscopic* to distinguish it from the other, is definitely a bad one and not calculated to give the best description of physical phenomena. A good deal may be written on this question, but the author believes it boils down in the last analysis to a question of taste. No one doubts, for example, that thermodynamics is an extremely useful tool for the solution of many problems. There are some problems, however, for which it provides no definite answer; e.g., that of radiation. We do not wish to forgo a solution, so we look for it where we think we may find it, i.e., by providing a structure for matter. The same kind of situation has arisen in most branches of physics and will presumably arise in all as experimental investigations become more searching and detailed.

PROBABILITY AND STATISTICS

The introduction of atomic theory into physics and the attempt to frame this theory along dynamical lines has led, of course, to the necessity of using the statistical method in physics and this in turn implies the use of probability calculations. Of all mathematical concepts used in physics that of probability is without question the most difficult to define with precision and the most fruitful in stirring up controversy over meaning. In a way this is a curious situation, since few physicists really question the value of statistical theories in physics. It is a particularly difficult situation for the thoroughgoing operationalist, for on his point of view there is no meaning to probability unless there is an operation which will assign a number to it. Now there is no operational way of assigning a probability to a single event. Suppose the event in question is the turning up of a six when a single die is cast. What physical operation can we carry out to assign a probability to this? One might reply that we can cast the die a great many times and take the ratio of the number of times a six turns up to the total number of throws. This is certainly an operation or a set of operations, if one prefers. However, strictly speaking, it says nothing about a single event: it merely gives the frequency with which that event appears among a whole set of events. To say that this number shall actually represent the probability of the *single* event of turning up a six is to

introduce a bit of conceptualization which certainly lies outside the strict criteria of operationalism.

It is therefore not surprising to find an operationalist like Bridgman emphasizing that probability has nothing to do with the single isolated physical event. When he thereupon proceeds to imply that we can never be sure that there is any logical foundation for the use of probability in physical theories, we find it necessary, however, to protest. Certainly there may be grave question about the fundamental meaning of probability but there need be no question about the definite way in which the idea is used in the construction of a physical theory. From this point of view the problem is quite on a par with other fundamental concepts, (e.g., space and time), used in the framing of theories.

Consider for a moment the kinetic theory of gases. We have to begin by assuming the existence of molecules. We must further suppose that the ordinary macroscopic amount of gas contains an enormous number of the molecules moving about in all directions with a wide range of velocities. The molecules, at any rate in the simpler form of the theory, are taken to be single mass particles moving in accordance with the principles of mechanics, obeying in their collisions with each other and the walls of the containing vessel the laws of the conservation of momentum and energy. In principle, therefore, if we knew at any instant the position and velocity of every molecule we could calculate its position and velocity at any past or future time. In so far as the gas is merely the aggregate of these particles, all its properties should be derivable from the result of the above calculation and the kinetic theory in principle would be nothing but an application of the mechanics of aggregates of particles. The catch is that we can never hope to *know* exactly the initial conditions of the system, i.e., the position and velocity of every molecule. What then do we do? We might prefer to give up the problem as hopeless. Actually we decide not to concentrate our attention on the individual molecules, but to assume a certain *average distribution* with respect to position and velocity and work with this throughout. It is to be emphasized that whatever distribution is assumed is bound to be arbitrary and can have *no a priori* justification. We may, indeed, be led to it by what we may please to call "probability" considerations, but these are intended merely to make plausible what is actually arbitrary. The point is that the situation is not essentially different in the construction of *any* physical theory. We may be led to our postulates by plausibility considerations of various kinds but that can never make them *a priori* true, nor ought we to delude our-

selves into thinking that they are anything but arbitrary and justifiable only by the agreement between experimental observations and the deductions from the theory.

It is clear that from this point of view, which is tacitly inherent in every physical theory, there is absolutely no foundation for casting aspersions on the use of probability ideas in the kinetic theory on the ground that probability has no application to concrete physical situations. This conclusion has indeed been reinforced by the success which has accompanied the introduction of the new types of quantum statistical distribution. Here there is certainly no question of the *a priori* justification of the fundamental idea of the distribution. All we know is that one type of distribution, viz., the so-called Maxwellian, *works* for ordinary gases under usual conditions, while a quite different type, that of Bose-Einstein, works for a gas composed of photons, and still a third type, due to Fermi and Dirac, works for an electron gas. We might, of course, be tempted to ask *why* these different methods of distribution work for different kinds of gases and this would indeed bring us face to face with all the embarrassing fundamental questions connected with the use of probability in physics. But it is just this little question "why" that we are forbidden to ask in physical methodology: it leads to an endless regression and is therefore meaningless. There is fundamentally no more meaning in asking why the various distribution laws of statistics apply to the corresponding types of gas than in asking why the principles of mechanics apply to the motion of macroscopic particles. This attitude, naturally, will not prevent us from seeking *relations* among the various statistical distributions which may, for example, exhibit them as special cases of one general type of distribution: theoretical physics progresses by seeking for and establishing just such kinds of relations. But this is a quite different thing from insisting on *a priori* justification for each type of probability assumption used and then saying that since this justification can not be found by operational means, the logical structure of the theory falls to the ground.

When all is said the only real use which physics makes of probability is in the calculation of *average* values for comparison with experiment. Physics has no interest in the probability attached to a given concrete event. It is true that among other averages it is also interested in the *average fluctuations* of quantities from their average values. Thus, in the case of an ordinary gas far from its critical point, contained in a box, when equilibrium is attained the average density is uniform throughout

the box. The kinetic theory, however, allows in any sub-volume of the box fluctuations from the average. It is conceivable that these fluctuations may be large. If they were large and if they persisted, we should certainly expect to detect them experimentally (e.g., in the optical behavior of the gas). When, however, we take an average (the particular kind of average is not important) of the fluctuation we find, when the sub-volume is large compared with the wave length of visible light, the average is extremely small and indeed far below the possibility of measurement. It is only when the volume is of the order of the wave length of visible light or less and the gas is near its critical point that the average fluctuation is of such size as to stand any chance of detection. In fact, it is only in the latter case that it *is* detected experimentally. The physicist naturally concludes that the theory has met successfully at any rate *one* test. How does the operationalist view this matter? He is much disturbed by the fact that the theory actually *allows* the existence of the rare fluctuation, even if with so small a probability that on the average it will never be observed. He takes the ground that the fact that the rare fluctuation is never observed is a very weak verification of the theory predicting the improbability of its occurrence. If this were the only possible kind of verification of the theory, he would be on pretty safe ground in distrusting the whole business. Certainly no physicist will be contented with a theory which merely predicts that certain things will not happen and then duly finds that they actually do not happen. Fortunately statistical theory in physics is not reduced to this impoverished state. It makes positive predictions and to the extent that these predictions accord with the facts, we can be satisfied with the theory. We should indeed be in trouble if the theory were to assign a *large* value to the average fluctuation and no physical observation could be associated with this value. We should then be justified in looking out for a better theory and should undoubtedly proceed to do so.

RÉSUMÉ

The critique of operationalism in physics presented in the preceding pages can not be considered complete, since no discussion has been given of its relation to the theory of relativity and no really adequate account of its attitude to quantum mechanics. These, however, are matters for a separate article. It is hope that the present paper has made clear that though operationalism is a fascinating viewpoint and casts valuable light on the problems of physics, it betrays certain weak-

nesses which make its thoroughgoing application to physics of questionable utility. Certainly it would demand the complete scrapping of the well-recognized methodology of physics. The success already achieved by the latter will render most physicists cautious about sacrificing theoretical physics on the altar of operationalism.

Brown University
Providence, R. I.

Symbolic Logic and "Embedding Language"

BY

EDWARD O. SISSON

(For convenience of reference:
Whitehead and Russell, *Principia Mathematica*, Vol. I (2nd Edition, 1925) PM
Lewis and Langford, *Symbolic Logic* (New York, 1932).....LL
Rudolph Carnap, *Abriss der Logistik* (Wien, 1929).....Carnap

I

MR. A. F. BENTLEY in his *Linguistic Analysis of Mathematics* has attacked the problem of the "embedding language" of mathematics; "This essay" we read in the Foreword, "deals with the language of mathematics, including not only the mathematical symbols, but also those immediately surrounding forms of expressions and assertions through which the symbols are developed, communicated and interpreted. The writer seeks to establish a firm construction for this embedding language." Inevitably, in the first instance this embedding language must be, as he puts it, "every-day language"; but this is sadly inadequate: "The every-day language reeks with philosophies—the absolutisms of pointing. It shatters at every touch of advancing knowledge. At its heart is paradox . . . Mathematicians know this. Yet they feel ever the compulsion to interpret their mathematics in terms of every-day language. So proceeding, their harvest is super-paradox." Yet the writer recognizes after all that "every-day language is the basal medium of communication between men," even when we seek to "extend standards of symbolic consistency into the regions of the embedding language."

Thus the work has the very great merit of perceiving and unambiguously stating that an examination of language itself must precede all sorts of further analyses in which language is used—which would certainly include both mathematics and logic: we read, set out emphatically as "Rule I":

> "Every exact analysis, in, of, or by means of language, rests in preliminary provisional dissection and organization of linguistic materials." (P. 38)

Systems of symbolic logic manifestly also must be "developed, communicated and interpreted" by means of the "embedding language." Neither PM nor LL explicitly attacks this problem, but begin their exposition without any "provisional dissection and organization of linguistic materials." Clearly the embedding language for both mathematics and logic is propositional, and so subject to all the conditions and limitations of propositional expression. When we plunge into PM or LL, we find ourselves in a stream of discourse expressed in ordinary language, composed of words in linear concatenated order, in which each word and group of words must be "understood," which means that the reader must get in his mind the same "ideas" which the writer had in mind. Long before the acknowledged "primitive ideas" and "primitive propositions" are reached, other ideas have been swarming into our minds, at least undefined and possibly primitive.

II

If it be said that this is mere captiousness, in that all "explanations" must begin with unexplained words, the answer is that the inquiry must begin on the level of the widest and clearest general understanding, namely the "embedding language." The fact that this embedding language "reeks with philosophies," and has paradox at its heart, so far from releasing us from investigating it is of course the imperative demand for a prior inquiry which shall be linguistic as well as logical. The method of such an inquiry is to examine language in its most elementary processes, exactly as Aristotle set out to do in the *Organon*. Points of full and clear agreement must be found in the common experience of intelligent persons communicating on this elementary level. Such common linguistic experience arises out of and operates within the broad common experience of life.

This also means that it is unsafe and likely to lead to fallacy if we insist upon postulating *freely* what ideas and propositions shall be taken as primitive. For the "embedding language" is a hard reality, irre-

movable from the empirical processes of all our communicated mental life, and the essential facts of that embedding language inexorably condition our postulation. It is a striking fact that the sets of primitive ideas and propositions vary materially, e.g., as between PM, LL, and Carnap, in spite of Carnap's statement that his system is based upon PM.

III

PM and LL both take "*propositions*" as primitive idea, the former specifying "elementary propositions,"—with a very complex explanation, (Pp. xvii, 91, (1)), while LL simply has "propositions" (P. 123). From the point of view of linguistic inquiry this is nothing but the play without Hamlet. The *R* in the symbolic expression of a simple proposition, aRb, is an indispensable factor in all possible forces of the proposition, including the most important of all, implication. When we pass from the examination of the proposition to the problem of dialectic, it is difficult to avoid the conviction that the lines of *inference* are determined by the structure and content of the proposition, its two terms and the relation between them.

Interesting it is that LL explicitly notes the area of inference from a single proposition, which the symbolic procedure passes over untreated and uninvestigated. (P. 122) But the reasonable view is that all mediate inference is also rigorously conditioned by the intrinsic individual form of the propositions which serve as premises.

Curiously also, Carnap seems to define proposition (*Aussage* surely must be just proposition). (P. 3, 2b.)

Carnap seems to take "True" and "False" as primitive; at least he says that they are "undefinierbare." (ibid.) But PM (Pp. 41–5) and LL (Chapter VII) at least seem to take a different view. In any case "True" and "False" are not explicitly taken as primitive ideas by either PM or LL.

Carnap starts his system thus: "Zeichen bezeichnen entweder etwas Bestimmtes oder etwas Unbestimmtes . . ." (P. 3, 2a). The first two words (or indeed either one of them) involve the whole doctrine of symbolism, the major problem of which is "How *do* symbols signify?" It is hard to see how any logic which evades this problem can possibly keep open its lines of communication with its "embedding language." In 2c we read: "Jedes Zeichen, das eine bestimmte Bedeutung hat, bezeichnet entweder eine Aussage oder einen 'Gegenstand'." The vital question whether the "Gegenstand" is a "real thing" or "a thing

in the mind" is here blurred. But a few lines down we have a definition of *Begriff*—"etwas, das von einem Gegenstand aussgesagt werden kann" —surely in a proposition. But is not "Begriff," also, in all ordinary usage, the mental representation of a "Gegenstand"?

IV

Concerning symbolism in general and language in particular the most basic fact is that the symbol has two parts, one public, a sound or mark, utterly physical and objective, the other private in the sense of being in a mind. These two are manifestly what are commonly called word and meaning, or more generally symbol and meaning. But of course a word or any other symbol without a meaning is no word or symbol at all but only a noise or form. In speech a word is explicitly a sound with meaning. It would be both stupid and impertinent to labor this except that this duality of the symbol is absolutely basic and at the same time elusive in the highest degree. In order to speak clearly I propose for the present discussion to call the external public side of the symbol, the noise or mark, *obverse*, and the mental private side *reverse*; in the symbol as in the coin, the obverse presents itself to view, while the reverse is hidden; only the coin can be turned over while the symbol cannot.

Of course everybody who thinks about language at all knows this familiar fact, at least while he is thinking about it. Russell aptly and emphatically says the obverse (in our sense) is "an inkblot on paper of recognizable form." Any escape into "pure symbolism" in the sense of symbols without meaning, would arrive in a world of pure obverses, that is noises or inkblots. Bentley falls into this error quite plainly in his "Semantic Postulate." (Pp. 20–27) But at least he resolutely attacks the problem and does not pass it in the dark. The reverse is strictly *in the mind*. Language has no direct control over external things, but can only refer or correspond to them. All this Aristotle saw (see *De interpretatione*, 16 a3) and common sense sees. We can state this and use it in logico-linguistic theory without any metaphysical implications. Hence the most "abstract" symbol must have a reverse, its abstractness lying only in the fact that the reverse is *only in the mind* and does not "correspond" to anything in the "real" world. But the most concrete symbol also has its reverse strictly *in the mind*,—with the added fact that the "thing in the mind" in this case corresponds to a "Real" thing.

PM has a chapter on "Incomplete Symbols" (Introduction, Chap. III). "By an 'incomplete' symbol we mean a symbol which is not

supposed to have any meaning in isolation, but is only defined in certain contexts." (P. 66) Apparently the only complete symbols are *proper names*, as, for example, "Socrates." But even "Socrates," in isolation, cannot be said to be defined, for there are many individuals who bear or have borne that name. There is even a worse indefiniteness yet, in that, as can readily be seen, every word and also every symbol, must perforce be the name of three radically diverse things,—the "meaning," in this case perhaps a famous Greek;—the word as a word, "Socrates is a noun";—and the mere noise, as in "Socrates is a euphonious word."

This idea of specially incomplete symbols leads in the same paragraph to what seems to any unsophisticated mind an absurd statement, that the proposition "The round-square does not exist" cannot be regarded as "denying the existence of a certain object called 'the round-square.' For if there were such an object, it would exist." Is it not in truth exactly the *object* whose existence is denied? The thing which is not denied, and cannot be denied without reducing the proposition to meaninglessness, is the "thing-in-the-mind" which is the reverse of the name "round-square." The doctrine of obverse, reverse, and correspondence, (as held by Aristotle) is all required to give sense to any statement as to "existence," in the sense of something in a real world, "outside" of the mind. It is to be noted again that this linguistic doctrine neither implies any metaphysics nor can be refuted by any metaphysics. But it is rigorously demanded in all examination of symbols.

(The discussion of the proposition as an incomplete symbol (PM, P. 44) may well be examined in the light of the above considerations).

V

PM notably arrives at certain astounding conclusions:—that "A true proposition is implied by any proposition" (*2.02, P. 99) and (2.21) "A false proposition implies any proposition." This is followed quite naturally by the definition of equivalence,—"When each of two propositions implies the other, we say the two are equivalent. . . . It is obvious that two propositions are equivalent when, and only when, both are true or both false." (P. 115); see also *5.1, P. 123, and *5.21, P. 124.) It is to be noted that LL calls these paradoxes and sets up a system to avoid them (P. 122). It is hard for a nonlogistic mind to make sense out of either: the first seems to imply that if one knew any one true proposition, he might find his way to all true propositions; but alas—and this may not be quite so absurd—if one entertained one false proposition.

he might be led to any and all propositions, true and false. The second proposition, 2.21, is susceptible of a humorous interpretation, that if one believes that a high tariff benefits the consumer, he may as well believe also that the moon is made of green cheese.[1]

Without pretending to judge the logistic procedure which arrives at these propositions, one may express the suspicion that they are ultimately due to the refusal to analyze the proposition and seek to discover the bearing of its own structure and function upon the force of implication. Certainly in all ordinary thinking, including scientific thinking, the logical force of any proposition arises from the pattern of its *terms* and the *relation* asserted between them.

VI

This is perhaps the place to note a positive doctrine which seems to be justified by the examination of linguistic expression, that absolutely all meaning and, therefore, all logical force reside in the *reverse* of terms and propositions, and not in the least in the *obverse*, which is only noises or marks in themselves utterly irrelevant to the reverse, held to it and signifying it solely by "convention,"—the agreement of the communicating minds. The deceitfulness of words is an old story, none the less true for having been often told: we deceive ourselves by words without being aware of it, and we may juggle words to mislead others. "Symbols" in the narrower sense as used in mathematics and logistics (and in many other sciences and pursuits) are superior in *precision* and *convenience*, but even the most perfect symbols do not in the least escape from or transcend the basic peril of all symbols,—that only the *obverse*, the noise or mark, is public and accessible to both communicants; while the *reverse*, the "meaning," is inexorably private, in the mind, first of speaker and then of hearer. This is the enigmatical quality which eternally besets symbolic communication.

This leads to a question as to "equivalent" propositions: can each of two propositions imply each other, except when they have the same *reverse*, differing only in the outer form? And is this the sense in which all logic is declared to be "tautological"? (LL. P. 24). PM 659

[1] LL declares "... there is no such thing as *the* conclusion from any premise or any set of premises; given any premises whatever, there are an infinite number of conclusions which may validly be drawn." (P.70) However, this is given under the head of the algebra of logic, and perhaps may not apply generally. If it does so apply it is as destructive to the hopes of achieving knowledge by logical means as are the "paradoxes" of PM.

VII

Much attention is given in symbolic logic to certain logical paradoxes, and elaborate "solutions" are offered. Some of these certainly yield to simple linguistic analysis without the use of symbols; if some, perhaps all might. Take "the oldest contradiction of the kind," dealt with by both PM (P. 60) and LL (P. 438)—that involving Epimenides the Cretan. The original form of the "paradox" is hardly more than a joke and far too loose to be dealt with logically. PM reduces it to "the man who says 'I am lying'," still a hopelessly loose form; LL does better with "This proposition is false." Linguistic examination analyzes the proposition into two terms, each being a name with denotation, and a "linking" or relation between them. Consequently, when the paradox is started with the words "This proposition," we ask, "What proposition?", that is, what is the denotation of the first term? In order to make a paradox, the answer has to be, "The proposition about to be uttered"; but manifestly the whole proposition, about to be uttered, cannot be one term in the relation which is to be expressed by the proposition when it is uttered. Indeed, the word "this" is a very "incomplete" symbol, requiring some sort of gesture or pointing to give it any significance whatever. The nature of linguistic expression would require the speaker to say something like this: "The proposition which I am going to utter is false,"—then the "false" proposition would still be waiting to be spoken, and when spoken could be judged true or false on its own merits; this judgment would then determine the truth or falsity of the original announcement. Without some such explicit pointing to accompany the "this," the phrase "this proposition" is, to use Russell's own words, a set of meaningless noises (or inkblots on paper) and consequently the total sentence is not a logical paradox but just nonsense.

The same simple analysis holds for the elaborate form of the same paradox given in LL. P. 444–5.

Take (5) on P. 61 of PM: "the least integer not nameable in fewer than nineteen syllables," which is 111, 777. "But," we read, " 'the least integer not nameable in fewer than nineteen syllables' is itself a name consisting of eighteen syllables." One must confess to astonishment at the puerile character of the supposed paradox, since it depends entirely upon a simple ambiguity in one word, "nameable," which in the first proposition means "nameable by the standard English numerals expressed in words," while in the second it must mean "nameable in any way whatsoever," as for example by the phrase in the original

statement. Why not make the paradox far bigger, by saying, "Let X equal 111,777," and thus "naming" 111,777 in one single symbol? The paradox would be exactly the same. The example is worthy of a place in the *Cratylus*, where everything has one name belonging to it by divine ordinance or law of nature.

VIII

The failure to analyze not only the proposition itself, but the situation in which propositions arise, leads to great confusion in one of the most important and promising passages of PM, Section III of Chapter II of the Introduction, Pp. 41–7. We read (P. 43) "This judgment of perception, considered as an actual occurrence, is a relation of four terms, namely *a* and *b* and *R* and the percipient." When PM looks at a judgment "considered as an actual occurrence," it is on the verge of a sound analysis and dissection of the rise of linguistic expressions, and this is the virtue of intention in this section as a whole. But to call *a* and *b* and *R* and the percipient all "terms" is at the least to bar any clear terminology either for the situation or for the proposition which is to spring from it. The next statement is sound, except that it still confuses the use of "term"; "The perception, on the contrary, is a relation of two terms, namely '*a*-in-the-relation-*R*-to-*b*,' and the percipient." Yet in the next paragraph we read: ". . . a judgment does not have a single object, namely the proposition, but has several interrelated objects . . . when we judge (say) 'this is red,' what occurs is a relation of three terms, the mind, and 'this,' and red." To this we submit, first, that it is exactly the entity of the whole proposition and not the "terms" of the proposition which is the object of the judgment and the essence of the proposition; and, a minor but significant point, that one "term" has dropped out in this statement, the *R* which holds between 'this' and 'red.' The quartet of the first statement has shrunk to a trio. Clear-cut examination of the proposition itself, as *held in or by* the "percipient" mind, would avert the confusion; mere consideration of efficient diction forbids calling the mind a "term," and sound diction promotes sound substance.

Carnap makes a remark which strikingly expresses the sound view; "Mit 'Aussage' ist nicht der historische Akt des Sprechens, Denkens, Vorstellens gemeint, sondern der zeitlose Inhalt." (P. 3, 2b) It is this "zeitlose Inhalt" as an entity which constitutes a proposition, and which the mind judges to be true or false; and the minimum typical essence of this "Inhalt" is symbolized by *aRb*,—two terms, both significant, linked in a relation, also significant.

If "minds" are to be taken into account then *two* minds must be reckoned with,—speaker's mind and hearer's mind. Even when one person "communicates" with himself, in processes of reflection, two functions occur, the formulation of the proposition and the acceptance of the proposition.

On this general problem of the total situation in linguistic expression including the "minds" concerned, Alan H. Gardiner's *Theory of Speech and Language* (Oxford, 1932) contains a masterly account.

IX

PM offers *assertion* as primitive idea:

> "(3) Assertion. Any proposition may be either asserted or merely considered. . . . The sign '⊢' is called the assertion sign; it may be read 'it is true that' (although philosophically this is not exactly what it means)." (P. 92)

We submit first that examination of linguistic form shows assertion to be quite outside of the pattern of the proposition and solely in the *intent of the speaker*. The "assertion sign" of symbolic logic which may be prefixed to the propositional symbol or omitted from it is quite in accord with this view. This however in no way invalidates the fact that in ordinary language every proposition carries its own assertion sign. This crops out in PM,—"*If I say* 'Caesar died,' *I* assert the proposition 'Caesar died,'" (italics mine). Only an "I" can assert, which is the same as to say with the grammarian, that "I" is the "first person or the speaker." Is not this exactly what assertion means, philosophically or any other way? Thus assertion is outside logic and in the pragmatic field.

But truth is something else and is in the propositum if anywhere,—good Russellian doctrine. F. C. S. Schiller is right when he says "Every proposition carries its own truth claim." "Caesar died" is either true or false totally regardless of any assertion whatsoever.

Does this mean that the real primitive idea here is truth, with its indispensable counterpart, falsehood? If so, the primitive idea gets in here at the side door. This seems to tally with Carnap's " 'Wahr' und 'falsch' sind undefinierbare Grundbegriffe."

This is not to belittle the distinction between a proposition as asserted and as merely considered. Ordinary language of course has its own familiar technique for this distinction in the interrogative form as distinguished from the declarative. And the assertion sign is an admirable and useful symbol. But assertion is not truth, and is not really "in logic."

X

What of *relation?* It is not included as primitive idea by either PM or LL. Carnap does not in the *Abriss* give any explicit statement of primitive ideas, but he does seem to stress relation. In his introductory section, entitled "Die Aufgabe der Logistik," we read "Die Logistik, und besonders ihr wichtigsten Teil, die Relationstheorie (oder Ordnungslehre) . . ." (P. 2). In section 2c, P. 4, we find a definition (or explanation?) of "Beziehungen." But it seems clear that "Beziehung" here is not synonymous with "Relation" in the other passage; this certainly sets a problem for the translator, and seems to raise logical perplexities.

Relation is certainly close to being a primitive idea. The insistence upon the view that *linkings* are *significant* and *variable* is quite antagonistic to the Aristotelian scheme, and in general, consonant with the procedures of symbolism. It is linking which completes the intrinsic pattern or content of the proposition, and it is this pattern that provides all the logical force of the proposition. A critique of the proposition should be followed by a critique of dialectic, which would trace the courses of inference; these courses would be function of the intrinsic pattern of "terms and linkings" in the propositions concerned.

That relation is basic and paramount in any sound view of language, and especially of the proposition, with which logic is solely concerned, seems clear from critical examination of the technique of linguistic communication. The simple proposition is two terms linked in a certain relation, as symbolic logic precisely indicates by its formula *SrP*. This triadic pattern is the absolute minimum of proposition, as Aristotle saw when he said that the proposition "says something about something." The three elements, two terms and one relation, are all *significant* and *variable.* While the terms are in themselves static, non-directional, the relation is moving and directional. Terms, being in themselves names, standing for foci of attention, are comparatively easy to deal with and have yielded to linguistic and logical examination. Relations, being between terms, and, as Aristotle saw, not conceivable apart from the terms, are far more subtle and baffle the analysing mind. Aristotle himself was partly deceived by thinking the nonsignificant "is" to be the "copula" or relation-bearer, and syllogistic logic was pervaded with this fallacy.

Relation cannot be a primitive idea in the sense of being accepted without analysis. Yet it is infinitely variable and manifold and intri-

cately complicated by its very relational essence. Is it possible to apply to the problem of relation some of the acuteness and precision which is so characteristic a virtue in the momentous enterprises of symbolic logic? It seems clear that the inquiry must take its start at the bottom with patient examination of the ultimate pattern and functioning of primary simple propositions, expressed, at least for the very beginning, in the words of ordinary language,—the "embedding language" for all higher levels of the logical system.

Reed College,
Portland, Oregon.

The Method of Postulates[1]

BY

EDWARD V. HUNTINGTON

THE subject to which I invite your attention this evening is a very recent development in mathematics which happens to be of great importance to philosophy.

The question may perhaps be raised why a terrifying topic like mathematics as introduced into an Institute of Philosophy. There are two answers to this question. In the first place my talk this evening will not be a "mathematical lecture" in any ordinary sense of the term, and no mathematical knowledge whatsoever will be presupposed. In the second place, even if it were to be a mathematical lecture, such an audience as is here assembled tonight would find nothing terrifying in the prospect.

We are all interested in the process of proving an assertion by some sort of argument. We all recognize that before we can carry out any satisfactory argument in any field, we must agree upon something as a starting point. Now "postulates" are simply the starting points of an argument, and the "method of postulates" is a method of clarifying and simplifying the whole process of argumentation. The fact that this method was first used in mathematics is an accident of history which should not blind us to the fact that the method itself has a range of applicability far wider than the field of mathematics.

Fortunately for both mathematics and philosophy, the old-fashioned delusion that mathematics is a very disagreeable subject has long since disappeared. Such a book, for example, as Tobias Dantzig's "Number, the Language of Science" has achieved a popularity which would have astonished our grandfathers. In Great Britain last year, L. Hogben's

[1] Outline of an illustrated lecture delivered at Brunswick, Maine, April 13, 1937, as part of the Institute of Philosophy held from April 6th to 16th at Bowdoin College.

book entitled "Mathematics for the Million" was actually one of the best sellers. This book (in spite of certain minor technical defects) was instantly appreciated as giving a vivid picture of the importance of mathematics in the development of civilization. A special American edition has just come off the press. Even the radio joins the general tendency. Mr. W. J. Cameron, the well-known speaker at the Ford Sunday evening hour, devoted his talk three weeks ago to Korzybski. Count Korzybski has devoted his whole life to a mathematical analysis of language. It is indeed a significant sign of the times that this once disliked subject should now be given a place on the radio at a Sunday evening concert. Surely mathematics is no longer the terrifying subject that it once was supposed to be.

In any case, as I have already stated, my talk this evening will not be a mathematical lecture. Although the subject of our discussion was first developed within the field of mathematics, the fundamental principles involved can be readily understood without any preliminary knowledge of that science. Moreover, as Professor C. J. Keyser has emphasized in his charming little book entitled "Thinking about Thinking," the method of postulates is "available in every field of thought, in the physical sciences, in the moral or social sciences, in all matters and situations where it is important for men and women to have logically organized bodies of doctrine to guide them and to save them from floundering in the conduct of life."

I will also add that the present moment seems to be a particularly appropriate time for a discussion of this method. The very latest book on mathematics which I have seen contains the following interesting statement: "The postulational method has come to be recognized as the clearest and most rigorous approach to any mathematical discipline. Today this tendency is all the rage." This quotation is from Professor E. T. Bell's entertaining and readable book called "Men of Mathematics," which was published only a month ago.

Again, the very latest book on philosophy which I have seen contains the following striking assertion regarding the modern method of postulates: "We are dealing here with a situation which is revolutionary, and this revolution is at the same time of such far reaching importance for all our thinking and philosophizing that we must come in touch with it if we are to philosophize soundly." This is taken from Professor Karl Schmidt's new book entitled "The Creative I and the Divine," which was also published within the last month.

These two statements, one from a noted mathematician, the other

from a noted philosopher, represent the latest up-to-the-minute opinion as to the importance and timeliness of our topic.

It would appear, therefore, to be eminently worth our while to try to find out what is really involved in this revolutionary method of postulates.

First, however, let me say just a word as to the historical background of our subject.

The first inkling of the postulational method dates back 2000 years, being found in Euclid's famous Elements of Geometry. The real significance of the method, however, was not appreciated by the Greeks. The first serious dream of a universal abstract language was published by Leibniz in 1666. The next landmark is the discovery of non-Euclidian geometry by Bolyai and Lobatschewski (about 1830). The next is the publication of George Boole's so-called "Laws of Thought" in 1854. It was not until about 1890, however, that the systematic development of postulational technique began with Peano and his school in Italy; and it was not until ten years later that Hilbert's "Foundations of Geometry" attracted wide attention to the method. The next landmark is the publication of "Principia Mathematica" by A. N. Whitehead and Bertrand Russell in 1910, which may be regarded as the starting-point of the present wide-spread interest in the postulational foundations of logic. (A complete bibliography of work in this field has just been published in the Journal of the newly formed Association for Symbolic Logic.) Within the last few years, so many papers have been published in the general field of postulate theory that it would be invidious to attempt to select any short list to be mentioned in this place.

Let us proceed at once therefore to the main business of the evening, which is to find out as briefly as we can what the method of postulates is all about.

As a first step toward understanding the main idea, I wish to point out the sharp contrast between two words: "axiom" and "postulate."

An axiom, as the word is commonly and properly used, means a self-evident truth, that is, a proposition the truth of which is so clear that it will be immediately accepted by every intelligent person. A postulate, on the other hand, is an assumption or hypothesis, that is, something agreed upon for the sake of argument. This distinction between axioms on the one hand and postulates on the other hand is fundamental and far-reaching.

The "method of axioms" represents the older habit of mind of philoso-

phers and scientists. The "method of postulates" represents the modern tendency.

According to the older habits of thought, modeled after a demonstration in Euclid, the only way to demonstrate a proposition in science or philosophy was to start with obvious axioms and then proceed by successive steps of logic until the desired proposition was proved as a theorem. This process implied a rigid hierarchy of scientific propositions, starting with the obvious axioms as one kind of thing, and ending with the demonstrated theorems as another kind of thing. An axiom could never be mistaken for a theorem and a theorem could never be mistaken for an axiom. The goal of the method was the attainment of ultimate truth.

The postulational method on the other hand sets itself a much more modest aim. It makes no use of self-evident axioms. It does indeed proceed from hypothesis to conclusion by processes of logic, but it makes no claim that its hypotheses are self-evident or that its conclusions are true. All that it claims is that if its hypotheses are valid then the conclusions will also be valid. The postulational method is the "If-Then" method; its results are never absolute, always hypothetical. In place of the imposing edifice presented by the older method of axioms, the modern method of postulates appears as a humble tool—useful, to be sure, but never ultimate.

The question may well be raised how such a modest tool can be of any use in modern philosophical thinking. As a matter of fact it is proving to be of the greatest possible use, not only in consolidating the results of our previous thinking, but also in expanding and directing our thought in new fields. As the first step toward understanding how these things can be, it is necessary, as I have said, to distinguish sharply between axioms on the one hand and postulates on the other.[2]

A second step in the understanding of the postulational method consists in the recognition that every set of postulates must be understood as belonging to a restricted field. There is a good old-fashioned word for this, namely, the universe of discourse. Before discussing any particular theory by the postulational method, it is necessary to agree in advance as to what universe of discourse we are going to talk about. For example, geometry is one universe. Economics is another universe. The financial field, the religious field, the artistic field constitute other universes, all of which must be kept distinct and separate.

[2] In technical mathematical literature, however, many writers use the word "axiom" in the sense here reserved for "postulate."

A third step is to recognize that within each universe of discourse the method of postulates is essentially a method of classifying various systems according to their functional properties, or logical structure.

This process of classification according to logical structure brings with it a great saving of mental effort. Thus, suppose we are trying to discover the properties of several apparently different systems. If we find that these systems all have the same logical structure, we can neglect the superficial differences between them, and carry on all our argumentation within the framework of an "abstract" system which embodies all the essential structure of each of the given "concrete" systems. Within this abstract system, we start with postulates and end with theorems. Any concrete system which is observed to have all the properties stated in the postulates will automatically have also all the properties stated in the theorems—without any further effort of observation on our part.

Thus the method of postulates may be briefly described as a tool for saving mental effort, through the simple device of classifying systems according to their logical structure.

This leads directly to our fourth step. In order to explain how effective use may be made of the new tool provided by the method of postulates, we must emphasize the abstract character of the symbols employed. Strange though it may appear, it is only with abstract symbols that logical reasoning can be carried on. Concrete facts are just concrete facts, all of them on what may be called the same level. Concrete facts do not arrange themselves in logical sequence starting with hypothesis and ending with conclusion. We do not speak of one concrete fact as a postulate and another concrete fact as a theorem. It is only within the structure of an abstract system that we can talk about postulates and theorems at all.

In order to show you what an "abstract system" really is, I am going to ask you to examine with me in detail a simple set of postulates which will serve as an example. This example will illustrate how a postulational system is actually constructed in practice.

We begin by calling to mind two concrete sets of facts of experience—one taken from the field of geometry, the other from the field of linguistics.

First let us consider a lot of regions, A, B, C, . . . in a square (including the null region and the whole square), as in a diagram. Let A′ denote the region complementary to A with respect to the square; that is, A′ is the portion of the square which lies outside of A. Let AB

denote the region common to both A and B. Then by direct observation of the diagram we observe the following facts.

A1. If A and B are regions, then AB is a region.
A2. If A is a region, then A′ is a region.
A3. AB = BA.
A4. (AB)C = A(BC).
A5. (A′B)′(A′B′)′ = A.
A6. If Z represents the null region, then AZ = Z.
A7. If U represents the whole square, then AU = A.
A8. If AB = A, then AB′ = Z.
A9. If AB′ = Z, then AB = A.
A10. If AB = A, then "A is within B"; and if "A is within B", then AB = A.
A11. If A is within B, and B is within C, then A is within C.
A12. AB is within A.
A13. If C is within A, and C is within B, then C is within AB.

And so on. All these properties are equally "obvious," and they may be written down in any order.

Next, we call to mind a second concrete situation.

Let P, Q, R, . . . be propositions, or statements, which may be either asserted as true, or merely considered. Let PQ = the joint proposition "P and Q," so that to assert the proposition PQ means to assert that both P and Q are true. Let P′ = the proposition contradictory to P, so that to assert the proposition P′ means to assert that P is false. Then by observation of the ordinary usages of language, we find the following concrete facts.

P1. If P is a proposition and Q is a proposition, then PQ is a proposition.
P2. If P is a proposition, then P′ is a proposition.
P3. PQ = QP.
P4. (PQ)R = P(QR).
P5. (P′Q)′(P′Q′)′ = P.
P6. If Z = absurdity (Z = PP′), then PZ = Z.
P7. If U = any necessarily true proposition, then PU = P.
P8. If PQ = P, then PQ′ = Z.
P9. If PQ′ = Z, then PQ = P.
P10. If PQ = P, then "P implies Q;" and if "P implies Q," then PQ = P.
P11. If P implies Q, and Q implies R, then P implies R.
P12. PQ implies P.

P13. If R implies P, and R implies Q, then R implies PQ.

And so forth. All these properties are equally "obvious," and they may be written down in any order.

We observe a striking similarity between these two concrete systems. The letters A, B, C, . . . in one correspond to the letters P, Q, R, . . . in the other. The "null region" in the first system corresponds to the "absurd proposition" in the second system. And the relation "A within B" of the first system corresponds to the relation "P implies Q" of the second system. In short, the two systems have the same logical structure.

We now proceed to set up an abstract system of which these two concrete systems will appear as instances.

Consider a set of entities, a, b, c, . . ., which we describe as belonging to a class K. (We do not care what these entities are. In one case they might be regions in a square; in another case they might be propositions.)

Let a × b, or simply ab, denote an entity determined by the two entities a and b according to any rule that we please. (For example, if a and b are regions in a square, ab might be the region common to the regions a and b; or if a and b are propositions, then ab might denote the joint proposition "a and b.")

Let a′ denote an entity determined by the entity a according to any rule we please. (For example, if a is a region in a square, then a′ might be the region complementary to a with respect to the square; or if a is a proposition, then a′ might be the proposition contradictory to a.)

We thus have before us an abstract "system (K, ×, ′)" where K is any class of elements, a, b, c, . . ., and × and ′ are any rules of combination among those elements.

What do we do next? We certainly cannot write down by direct observation the properties of this abstract system, as we did in the case of the regions in a square, or in the case of the propositions, because it hasn't got any such properties—as yet.

What we do is to write down "arbitrarily" a set of postulates which we desire to impose upon our system. More precisely stated, we make an agreement among ourselves that we shall not consider any systems (K, ×, ′) except those which have the properties we are about to set down. (Behind the scenes, I must confess, the choice of postulates is not quite as arbitrary as we pretend, since in the back of our minds we have the picture of some concrete example, like the regions in a square, or the propositions, to guide us in our choice! But theoretically all

we have to look out for is to be sure that our demands are not incompatible among themselves.)

Let the postulates we choose in this case be the following five.

1. If a and b are elements of K, then ab is an element of K.
2. If a is an element of K, then a′ is an element of K.
3. If a, b, etc. are elements of K, then ab = ba.
4. If a, b, c, etc. are elements of K, then (ab)c = a(bc).
5. If a, b, etc. are elements of K, then (a′b)′(a′b′)′ = a.

These five postulates define the kind of system (K, ×, ′) to which we agree to confine our attention.

Any system (K, ×, ′) which has these five properties we shall call a *Boolean system*, since systems of this kind were first suggested by George Boole.

We recognize at once that our old friends, the concrete system of regions in a square, with × and ′ specified as indicated above, and the concrete system of propositions, with × and ′ specified as our second example above, are instances of a Boolean system.

But why do we stop with five postulates? In each of the concrete examples we examined, we observed a dozen or more properties; why, in the abstract system, do we stop with five? The reason is that if we adopt the five postulates stated, all the remaining properties can be proved as theorems, and hence do not need to be explicitly mentioned. In other words any system (K, ×, ′) which is observed to have the five properties 1–5 will automatically have all the other properties, without any further effort of observation on our part. Stated in abstract terms, the theorems in question read as follows.

6. There is a unique element Z, such that aZ = Z; namely, Z = aa′.
7. There is a unique element U, such that aU = a; namely, U = (aa′)′.
8. If ab = a, then ab′ = Z.
9. If ab′ = Z, then ab = a.
10. Definition. The notation a < b shall mean: ab = a.
11. If a < b and b < c, then a < c.
12. If a, b, etc. are elements of K, then ab < a.
13. If c < a and c < b, then c < ab.

The important point to notice is that the proofs of these theorems (which I have not time to give here) do not depend at all upon any particular interpretation that may be given the abstract symbols (K, ×, ′). The proofs are entirely general, and apply once for all to any system (K, ×, ′) which satisfies Postulates 1–5). The only properties we need to "observe" are properties 1–5.

We note in passing that the concrete meaning to be attached to the *derived* symbol a < b depends of course on the meanings assigned to the *basic* symbols (K, ×, ′). Thus, if a, b, etc. are interpreted as regions in a square, with × and ′ specified as above, then a < b will naturally be interpreted as "a within B." If a, b, etc. are interpreted as propositions, with × and ′ specified as in our second example above, then a < b will naturally be interpreted as "a implies b."

Our abstract Boolean system (K, ×, ′) provides us with a compact description of an indefinite number of different concrete systems. Such a system as the regions in a square, with × and ′ specified as above, is just an instance of a Boolean system; and of course there are many other such instances; but the logical structure is the same for all, and the proofs of an unlimited number of theorems can be given once for all for all these systems, by simple algebraic processes.

The method of postulates thus achieves not only a great simplification but also a great generality.

Before leaving these illustrative examples, I should like to point out a further feature of the postulational method—a feature which, though not of basic importance, does add an attractive touch of elegance to the whole method. I refer to freedom from redundancy.

We wrote down a moment ago a set of five postulates as the defining properties of a Boolean system. The question immediately arises: How do we know that some one of these five postulates may not itself be provable from the other four? That is: How do we know that some one of these five postulates may not be redundant?

Well, methods have been devised by which we can give a definite answer to this question. In the present case it can be shown that no one of these five postulates can be deduced from the other four. In technical language we say that our five postulates are independent of each other. No one of them can be omitted without altering thereby the definition of a Boolean system. This property of independence is, as I say, something of a luxury. A set of postulates which is allowed to retain some redundant features may still be quite satisfactory for most purposes. But the process by which independence is established is so interesting in its own right that I shall give you a case of it. It is a curious business, this proving that something cannot be proved. No one thought of doing such a thing until comparatively modern times

The case I shall consider is the proof that Postulate 5 of our list is independent of the other four postulates of the list. The proof consists in exhibiting a concrete example of a system (K, ×, ′) which has the properties numbered 1, 2, 3, 4, but fails to have the property numbered 5.

Let a, b, c, ... = a class K consisting of all numbers, positive, negative, or zero. Let ab = the sum of the two numbers a and b. Let a′ = the negative of the number a.

This system (K, ×, ′) satisfies Postulates 1–4, as is readily seen. But it does not satisfy Postulate 5, since when a = 2 and b = 3, we have $(a'b)'(a'b')' = (-2+3)'+(-2-3)' = (-1)+(5) = 4$, which is not = 2.

Here then we have a concrete example of system (K, ×, ′) which has all the properties numbered 1, 2, 3, 4, but fails to have property 5. Hence we conclude that Postulate 5 cannot be proved from Postulates 1, 2, 3, and 4. For, if property 5 were a consequence of the other four properties, then every system which had the other four properties would also have property 5. But this, as the example shows, is not the case. Hence property 5 is not a consequence of the other four properties. We have thus shown that Postulate 5 is independent of the other four postulates of the list.

By exhibiting similar systems (K, ×, ′), we can show that *each* of the five postulates is independent of the other four.

The moral of this story is this: It is often possible to prove that something cannot be proved from something else.

[We may mention, in passing, the well known problem of the trisection of an angle as a case in point. Here the universe of discourse consists of all geometric constructions that can be made by the aid of ruler and compass alone. The problem is to give a construction which will divide any given angle into three equal parts. It has been definitely proved that under the restrictions stated no such construction is possible; and yet every year a lot of students will waste hours of time trying to solve this unsolvable problem, and many such alleged solutions find their way into the daily papers. The problem not only has not been solved; it never can be solved. Anyone who has caught the spirit of the method of postulates is always ready to consider, in regard to any given proposition, not only whether it is provable, but also whether it is definitely non-provable, from a given set of hypotheses.]

I should now like to show how an abstract Boolean system can throw some much-needed light on a confusing practical problem of ordinary language.

The practical problem to which I refer is concerned with the ambiguous little word "or." When we say "today is either Tuesday or Wednesday," we mean that it must be one or the other but cannot be both. But when we say: "Either John or William will call for you tonight," we mean that at least one of these two events will happen and possibly both. Latin was more definite than English on this point.

The two meanings of "or" were represented by two different words, namely "vel" and "aut." The statement "x vel y" meant "at least one of the two things x and y, not excluding the possibility of both"; but the statement "x aut y" meant "one and only one of the two things x and y." In our English language the two meanings of "or" are frequently confused. Let us see how this situation may be clarified by a study of an abstract Boolean system.

First, we introduce the definition $a \vee b = (a'b')'$, the letter v being suggested by the Latin word vel.

14. Definition. $a \vee b = (a'b')'$.

On the basis of this definition, the following theorems are provable from 1–5.

15. $a \vee Z = a$.
16. $a \vee U = U$.
17. $a \vee a' = U$.
18. $a(b \vee c) = ab \vee ac$.

These theorems and many like them hold true in any Boolean system. In particular, they will hold true in the system $(K, \times, ')$ in which a, b, c, . . . are interpreted as propositions, ab is interpreted as the joint proposition "a and b," and a′ is interpreted as the proposition which is contradictory to a. In this system the element $Z(=aa')$ = absurdity, while the element $U\ (=Z')$ = certainty.

In this system, the element $a \vee b$ is to be interpreted as follows. According to the definition, $a \vee b$ here means the contradictory of the statement $a'b'$. Now the statement $a'b'$ means the joint assertion of the contradictory of a and the contradictory of b, or in other words the assertion that both a and b are false. And to assert the contradictory of *this* proposition is to assert that at least one of the two elements a and b must be true. In short, the element $a \vee b$ must be interpreted as meaning "at least one of the two elements a and b." Hence, in the system considered, the element $a \vee b$ may be interpreted by the English phrase "a or b," provided we understand the word "or" in the sense of "at least one."

In the light of this interpretation, let us look at the interpretation of our new theorems. Theorem 15 says that the statement "a or absurdity" is equivalent to the statement "a" alone, which is obviously correct. Theorem 16 tells us that the statement "a or certainty" is equivalent to the statement "certainty" itself, which is also correct. Theorem 17 tells us that "either a or the contradictory of a" is equivalent to "certainty," which is also correct. And finally Theorem 18 tells us that the joint assertion of "a" and "b or c" is equivalent to the statement that "either ab or ac" must be true. A little thought will show that this also is correct.

If we try to untangle these statements by the use of ordinary English alone, we may easily become confused and find ourselves floundering in the ambiguities of language. It is very easy to make a blunder in dealing with the two meanings of the English word "or." On the other hand, if we state our definition and prove our theorems in the abstract language of the Boolean system, the danger of ambiguity is wholly removed and we know at every point exactly where we stand.

This may sound like a trivial case, but the situation is typical of thousands of much more difficult cases. The postulational method is the only known method for making clear and definite the structure and meaning of our language.

The subject of definitions deserves far more than passing reference. An abstract deductive system contains not only the basic symbols in terms of which the postulates are expressed, but also, in any practical case, a large number of derived symbols, in terms of which most of the later theorems are expressed. These derived symbols are introduced "by definition" to serve as abbreviations for certain combinations of the basic symbols.

Theorem 18 to which I have just referred is an illustration. In the universe of discourse which we were there considering, the basic symbols within the class K were only $\times$ and $'$; but this theorem contains a new symbol, v, which came into the system by way of definition, namely: $a \vee b = (a'b')'$. The question immediately arises: Why did anyone think of introducing this particular definition? Why was this particular combination of basic symbols $[(a'b')']$ regarded as of such special interest as to justify giving it a separate name?

Mathematics as such gives no answer to this question, which properly belongs in philosophy, because in the last analysis it must be recognized that the construction of any abstract deductive theory is the work of some active human agent. Systems do not grow of their own accord. They are created by human beings for the satisfaction of human needs. It is always a creative human being who picks out the combinations which (by a stroke of genius, if you will) he recognizes as important and fruitful. The quickest way to get at the real significance of any abstract deductive theory is to examine the definitions, or abbreviations, which the author of the theory has chosen to adopt.

I hope that these remarks about definitions may help to clear up the following very common misunderstanding. One often hears the statement that in any deductive system, all the theorems are implicitly contained in the postulates; or, in other words, that the process of deduction from the postulates never yields anything essentially new.

This impression, though widely prevalent, is quite erroneous. In every live deductive theory, novelties are constantly appearing, due to the introduction of new definitions by ingenious and original investigators. These new combinations are not implicitly contained in the postulates, but reflect the intelligent interest of some creative human agent. The postulates are like traffic laws; they prohibit traveling in wrong directions, but they do not provide any motive power.

For example, in Theorem 18, when once we have asked the question: "What is a(bvc) equal to"? the postulates prohibit us from giving any other answer than "ab v ac." But there is nothing in the postulates which requires us to ask the question. The origin of the question itself must be sought in a realm far deeper than the postulates themselves, namely in the realm of human volition.

Similar remarks apply to the choice of the postulates themselves. The postulates adopted as the basis of any system constitute the definition of that system, within its universe of discourse; and the selection of one definition rather than another as the subject for discussion is again a matter of human volition.

This much, however, is clear. The technique of the postulational method effects a great saving in mental effort, in connection with definitions, just as it does in connection with theorems. When one is engaged in classifying systems according to their logical structure, the fewer the number of fundamental concepts which have to be considered, the smaller will be the chance of errors of classification. When once an interpretation is decided upon for the basic symbols, the corresponding interpretation for the derived symbols, introduced by definition, is completely determined by the rules of logic, and is not subject to errors of observation.

Geometry provides a striking illustration. It has been shown that all the many concepts dealt with in that science can be defined in terms of a single class (say "spheres") and a single dyadic relation (the relation of "inclusion"). In other so-called exact sciences, like physics and chemistry, only a very few basic concepts are required, all the other concepts in the science being defined in terms of the fundamental ones (as "velocity" in terms of length and time, "work" in terms of force and distance, etc.). It is due largely to this fact that these sciences have acquired as much "exactness" as they have.

I should like now to ask you to recall with me the steps which we have taken together this evening in our attempt to understand the method of postulates.

First, we pointed out the distinction between "axioms" and "postulates." An axiom is a self-evident truth, taken, in the older habit of thought, as the necessary starting point for any deductive argument. A postulate, in the newer method, is an assumption or hypothesis, agreed upon as the starting point for an argument, but not in itself either true or false, because it involves variables, and may be true for some interpretations of the variables and false for other interpretations.

Secondly, we pointed out the importance of restricting ourselves, during any discussion, to a definite universe of discourse.

Thirdly, we pointed out that within a given universe the method of postulates aims at a classification of systems according to functional properties, or logical structure.

And fourthly, we saw that this aim can be carried out only with the aid of an abstract symbolism, in which each symbol has only such properties as we assign to it through our postulates, so as to be free from the confusing connotations of ordinary language.

Finally, in addition to the essential feature of compatibility, we noted as an aesthetic luxury the feature of non-redundancy; and we presented concrete examples as illustrations.

The method of postulates has achieved astounding results in mathematics and in the "exact sciences." In the opinion of many forward-looking thinkers, the progress of civilization may be said to depend directly on the success with which this same method is applied in the so-called social sciences. The hardest part of the work here is in making a clear-cut abstract determination of the appropriate universe of discourse, and in making a fruitful selection of the definitions to be adopted. But a good start is already being made in many of these fields, and with a more wide-spread appreciation of the simplicity and power of the method, rapid progress may be hoped for in the future.

In conclusion, I should like to read a brief quotation from Professor Whitehead, taken from a recent address of his which was published only a month ago. After apostrophizing Symbolic Logic as his "first love," he goes on to say: "When in the distant future the subject has expanded, so as to examine patterns depending on connections other than those of space, number, and quantity—when this expansion has occurred, I suggest that Symbolic Logic . . . will become the foundation of aesthetics. From that stage it will proceed to conquer ethics and theology."

With these prophetic words of Professor Whitehead we may leave the subject to the verdict of the future.

Harvard University.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

SYMPOSIUM OF UNIFIED SCIENCE

ORGANIZED BY THE INTERNATIONAL INSTITUTE FOR THE UNITY OF SCIENCE

International Encyclopedia of Unified Science:

The preparation of the *International Encyclopedia of Unified Science* which was referred to in this journal ("Unified Science and Its Encyclopedia," by Otto Neurath, vol. 4, 1937, pp. 265–277), has progressed satisfactorily and the authors and subjects of the first two introductory volumes can be announced:

Volume I

1. The Unity of Science, Introductions.
2. Theory of Signs, Charles W. Morris.
3. Mathematics and Logic, Rudolf Carnap.
4. Procedure of Empirical Science, Victor F. Lenzen.
5. Probability, Ernest Nagel.
6. Physics, Philipp Frank.
7. Cosmology, E. Finlay Freundlich.
8. Biology, Felix Mainx.
9. Formal Biology, Joseph H. Woodger.
10. Theory of Behavior, Egon Brunswik and Arne Ness.

Volume II

11. Social Science, Otto Neurath.
12. Empirical Axiology, John Dewey.
13. Sociology of Science, Louis Wirth.
14. General Linguistics, Manuel J. Andrade.
15. History of Science, Federigo Enriques.
16. History of Logic, Jan Lukasiewicz.
17. From Rationalism A Priori to Empiricism, Louis Rougier.
18. Problems of Empiricism, Walter Dubislav.
19. Logical Empiricism, Joergen Joergensen.
20. Bibliography, Carl G. Hempel, Joergensen, and Neurath.

These two introductory volumes will be entitled *Foundations of the Unity of Science*, and will give general introductions to the main fields which will be treated in detail in later volumes. The prospectus published by the University of Chicago Press gives the relation of the Encyclopedia to the Unity of Science movement. It states that the two volumes will be sold to advance subscribers for fifteen dollars. It also makes it clear that these two volumes constitute a complete and independent work, and that subscription to them does not involve any commitment to subscribe to the volumes which are to follow later.

The Organizing Committee of the Encyclopedia consists of Rudolf Carnap (Chicago), Philipp Frank (Prague), Joergen Joergensen (Copenhagen), Charles W. Morris (Chicago), Otto Neurath (The Hague), and Louis Rougier (Cairo and Besançon). An Advisory Committee includes among others the names of Egon Brunswik (Vienna and Berkeley), John Dewey (New York City), Federigo Enriques (Rome), Herbert Feigl (Iowa City), Waldemar Kaempffert (New York City), Victor F. Lenzen (Berkeley), William M. Malisoff (New York City), Ernest Nagel (New York City), Arne Ness (Oslo), L. Susan Stebbing (London), Edward C. Tolman (Berkeley), Joseph H. Woodger (London).

The general purpose of the *International Encyclopedia of Unified Science* will be to bring together material pertaining to the scientific enterprise as a whole. Its task will not be to present the detailed results of the special sciences but rather to stress the logical structure of the special sciences considered in relation to one another. It will therefore be concerned with the development of a unified scientific language, with the presentation of the results of logical analysis in the various sciences, with problems relevant to the foundations of the sciences, with the analysis and interrelationship of central scientific concepts, with questions of scientific procedure, and with the sense in which science forms a unified whole. Treatment of such matters will have a central place but will be supplemented by a presentation of the newer logical techniques and by material drawn from the history of scientific thought, the sociology of science, and the general theory of signs. The material will be presented in short monograph form. Otto Neurath is to act as Editor-in-Chief, with Rudolf Carnap and Charles W. Morris as Associate Editors.

The Encyclopedia project is approved by the International Congresses for the Unity of Science which expressed its willingness to cooperate in the execution of the plans. The University of Chicago Press will publish these first two introductory volumes during 1938 and 1939.

Most and perhaps all of the pamphlets will have appeared before the meeting of the International Congress for the Unity of Science which is to be held at Harvard University, September 5–10, 1939.

Announcements and discussions relevant to the Unity of Science movement and to the Encyclopedia will appear from time to time in these pages which the *Philosophy of Science* has kindly made available for this purpose.

The Board of Directors of the International Institute for the Unity of Science (267 Obrechtstraat, The Hague, Holland) welcomes cooperation in the furtherance of its purposes. The Unity of Science movement, the Congresses, and the Encyclopedia are natural correlaries of the sustained systematic progress of science and the increasing specialization of science. Together they aim to deal in the spirit of science with those matters which concern the scientific enterprise in its totality. Of their relevance and timeliness there can be no legitimate doubt. It is to be hoped that workers as well as those interested in the institution of science and in the spread of the scientific habit of mind will make them their own.

REVIEWS AND NOTES

YOGA

Yoga: A Scientific Evaluation. By Kovoor T. Behanan. The Macmillan Co., New York, 1937. Pp. xviii + 270.

This book purports to be the first scientific description and evaluation of Yoga. The author, a native of India and a Doctor of Philosophy at Yale University, learned the technique from India's leading Yogins, and then subjected its physical and psychological exercises to scientific scrutiny. He here reports his findings in connection with posture, breathing and concentration experiments, and compares the results on body and mind with what Western science has learned from its study.

Dr. Behanan is sceptical of the metaphysical principles which Oriental philosophy has adopted, even though he recognizes the physiological and psychological benefits of the practises. In his chapter on Yoga and Psychic Research he vacillates between his desire to make out a good case for Oriental ideas and his loyalty to the spirit of Western science. Only in the case of Dr. J. B. Rhine's work on *extra-sensory perception* does he find merit and promise. The reviewer therefore suggests that if Dr. Behanan wants to undertake a further piece of research, he next proceed to train some subjects in the discipline of Yoga concentration and then put them to work on the Duke University experiments. If Dr. Behanan really believes what he says on page 179 ("Rhine's results and conclusions are convincingly positive"), he has the opportunity to take the field of ESP work outside the controversial domain of statistics and probability.

OLIVER L. REISER.

University of Pittsburgh.

Author Index

Aldrich, V. C., 395
Bisbee, E., 371
Black, M., 427
Bohr, N., 289
Brunswik, E., 227
Carnap, R., 1
Chalmers, G. K., 75
Frank, P., 41
Hailperin, T., 125
Hofstadter, A., 390
Huntington, E. V., 482
Konvitz, M. R., 285
Leonard, H. S., 109
Lillie, R. S., 202
Lindsay, R. B., 151, 456
Malisoff, W. M., 122, 261, 287, 385
Margenau, H., 337
Menger, K., 299
Neurath, O., 265
Peirce, C. S., 109
Reiser, O. L., 392, 499
Richmond, D. A., 173
Rosenzweig, S., 96
Senior, J. K., 168
Sisson, E. O., 471
Tugwell, R. G., 412
Wiener, P. P., 387

Subject Index

Anatomy of science, 385
Arranging the sciences: an experiment, 261
Budget of paradoxes, 122
Causality and complementarity, 289
Complementarity, causality and, 289
Directive action and life, 202
Discussion (correspondence), 107, 278, 383, 496
Embedding language, symbolic logic and, 471
Empirical probability, 390
Encyclopaedia, unified science and its, 265
Life, directive action and, 202
Lodestone and the understanding of matter in seventeenth century England, 75
Logic and "Embedding language," 471
Logic, the new, 299
DesCartes' method of doubt, 395
Doubt, DesCartes' method of, 395
Mathematical applications, some dangers connected with, 168
Mathematical conception of nature, mechanical versus the, 41
Mathematical logic, foundations of probability in, 125
Meaning, testability and, 1
Meaning of simplicity in physics, 151
Measurement of time: a first chapter of physics, 173
Mechanical versus the mathematical conception of nature, 41
Memory continuum, 392
Objective relations, psychology as a science of, 227
Objectivity in the social sciences, 371
Operationalism in physics, a critique of, 456
Philosophical history, 387
Physical theory, critical points in modern, 337
Physics, the meaning of simplicity in, 151
Postulates, the method of, 482
Pragmatism and scientific metaphysics of, 109
Probability in mathematical logic, 125
Psychology as a science of objective relations, 227
Psychology, schools of, 96
Sciences, arranging the, 261
Simplicity in physics, the meaning of, 151
Social sciences, objectivity in the, 371
Testability and meaning, 1
Time, the measurement of, 173
Troublesome "X", on the, 412
Unified science and its encyclopaedia, 265
Vagueness, 427
Yoga, 499

PHILOSOPHY OF SCIENCE

Manuscripts should be sent to William Marias Malisoff, 3965 Sedgwick Ave., New York, N. Y. English manuscripts may be sent to Miss L. Susan Stebbing, University of London.

Twenty-five reprints of articles will be furnished gratis to contributors when ordered in advance. An order slip, with table of cost of additional reprints, is sent with proof.

PHILOSOPHY OF SCIENCE is published quarterly, appearing January, April, July and October.

Correspondence concerning business matters should be addressed to The Williams & Wilkins Company, Mount Royal and Guilford Avenues, Baltimore, U. S. A.

Subscription price: $5.00 per volume, $5.50 outside the postal union. Prices are net, postpaid.

Claims for copies lost in the mails must be received within 30 days (90 days, foreign) of the date of issue. Changes of address must be received at least two weeks in advance of issue.

New subscriptions and renewals are entered to begin with the first issue of the current volume. Should any issue of the current volume be out of print at the time the subscription order is received, the pro-rata value will be credited to the next volume, and the renewal invoice adjusted accordingly.

Subscriptions should be renewed promptly—To avoid a break in your series, subscriptions should be renewed promptly. The publishers cannot guarantee to supply back issues on belated renewals.

Subscriptions, new or renewal, should be sent to the Publishers or to Agents listed below.

AGENTS

For Argentina and Uruguay: Beutelspacher y Cia, Sarmiento 815, Buenos Aires.

For Australia: Angus & Robertson, Limited, 89–95 Castlereagh Street, Sydney.

For Belgium: Henri Lamertin, 58 Rue Coudenberg, Bruxelles.

For the British Empire, except Australia and Canada: Baillière, Tindall & Cox, 8 Henrietta St., Covent Garden, WC. 2, London, England.

For Canada: Wm. Dawson & Sons, Ltd., 91 Queen Street East, Toronto.

For China: Commercial Press, Ltd., Paoshan Road, Shanghai, China.

For Denmark: H. Hagerup's Boghandel, Gothersgade 30, Kjöbenhavn.

For France: Emile Bougault, 48 Rue des Ecoles, Paris.

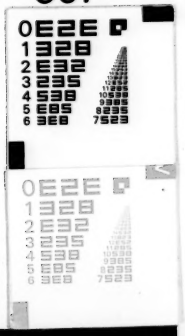
For Germany: B. Westermann Co., Inc., Zimmerstrasse 35–41, Berlin SW-68, Germany.

For Holland: Scheltema & Holkema, Rokin 74–76, Amsterdam.

For Japan and Korea: Maruzen Company, Ltd. (Maruzen-Kabushiki-Kaisha), 6 Nihonbashi Tori-Nichome, Tokyo; Fukuoka, Osaka, Kyoto, and Sendai, Japan.

For Spain: Ruiz Hermanos, Plaza de Santa Ana, 13, Madrid.

THE WILLIAMS & WILKINS COMPANY
BALTIMORE, U. S. A.

PUBLISHERS OF: *Medicine, Journal of Urology, Journal of Pharmacology and Experimental Therapeutics, American Journal of Tropical Medicine, Journal of Immunology, Journal of Industrial Hygiene and Toxicology, Quarterly Review of Biology, Journal of Bacteriology, Annals of the Pickett-Thomson Research Laboratory, Chemical Reviews, Soil Science, Social Forces, Journal of Comparative Psychology, Mental Measurement Monographs, Occupational Therapy and Rehabilitation, The American Journal of Clinical Pathology, Journal of Physical Chemistry, Journal of the Biological Photographic Association, Medico-Legal and Criminological Review, Philosophy of Science, Journal of Organic Chemistry, Medical Classics.*

C51